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# The Scott Committee Report[1]

By D. H. MacGregor

The reference to this Committee was "to consider the conditions which should govern building and other constructional development in country areas consistently with the maintenance of agriculture, and in particular the factors affecting the location of industry, having regard to economic operation, part-time and seasonal employment, the well-being of rural communities and the preservation of rural amenities".

I consider this reference to be neutral between the claims of building and agriculture, and to be a natural result of the proposals of the Barlow Commission for the dispersal of industry. The Committee has, however, laid chief stress on the agricultural side, to the extent of making a definite agricultural policy, which goes far beyond "maintenance", a precedent condition of its main proposals. This policy is not argued in relation to our national economy as a whole, and the responsibility for it is placed upon the government. If the government does not adopt such a policy, therefore, a substantial part of the Committee's work is fruitless. The Report therefore is so framed as to create a strong pressure of predisposition for an agricultural policy which will keep its proposals practicable, and to that extent it steals a march on any further Committee which might be appointed with a definite reference to consider the reconstructive aspects of agriculture.

The second general aspect of the Report is in its planning proposals. Two members of the Committee have dissented from this part of the Report, as being outside the reference. The Committee wishes to have a planning Authority set up, for town and country, but it has itself come to conclusions which it clearly intends the planning body to accept. If the planning body is free, it may reconsider the detailed recommendations of this Report; if it is to be bound by them, it is not free. My interpretation is that the planning body is not to be free, and this dilemma arises because the Committee has again gone outside the terms of its reference. No doubt, there is such a thing as tied planning, but this Committee both recommends a flexible supervision over development, which is the best form of planning, and itself furnishes a plan some features of which are indispensable.

The Report is written in a most interesting style. The adoption at once of an attitude toward the land and rural life carries the reader forward on a surge of sympathy which is easily understood, for all the vocabulary is in its favour. Perhaps there is a primeval instinct to which we readily revert, and which has all the best words on its side, better than the "use of scarce resources" or the "marginal advantage". But the cool analysis of Mr. Dennison, in a dissenting Report, is an economists' document; and, if his vocabulary had to be more severe, and his phrases less poetical, he has skilfully called "steady" to the argument and properly framed it. The two Reports must be read together; it is not right to speak of "the" Scott Report. I am more influenced by Mr. Dennison, but I do not expect everyone to be; the instinct of reversion to the land is strong against the limitations of the conditions of life of an "advanced" or "civilised" nation.

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[1] *Report of the Committee on Land Utilisation in Rural Areas.* August, 1942. Cmd. 6378.

The Report of the Committee may be divided as follows. There is a literary and historical Introduction about the nature and types of agricultural land in England and Wales, and their geographical distribution. A summary review is given of the open-field system, the Enclosures, the growth of villages and towns, and of the characteristic "patchwork" appearance of the countryside, of which they repeatedly speak with æsthetic approval. This leads to a further review of the changes which have often been described as the decline of agriculture, and particularly of arable cultivation. The "push" of the farmer in order to reduce his costs was combined with the "pull" of the cities for a number of reasons which have long been well known. The tendency of this account is to predispose the reader to a sympathetic attitude toward agriculture, and it says nothing about the significance of the decline in relation to our national economy as a whole. The Committee could have assumed that these general facts about agriculture were quite public.

Next, they consider the nature of the "push" of the cities into the country, for industrial or residential suburbs, or for more sporadic building. The account which they proceed to balance includes the magnitude of the encroachment for twelve years before the war by all kinds of building, the further effect of the threat of development in keeping land idle for a time, the break-up of farming units by roads and buildings, the eyesores, the noxious fumes, and so forth. They add to this side of the account the rural discontent which may arise when more highly paid occupations are close to the homes of rural workers, the disturbance due to the clash of mentalities, and the inconsiderateness of city people in their attitude and conduct toward the countryman. On the other side they place any advantages which come from closer union and mutual understanding, the creation of alternative occupations for wives and daughters, and the bringing of social amenities resulting therefrom. These are the obvious facts, which may enable some cancelling out, but between which there is no measure; but the Committee think it is "clear" that more harm than good has been done by encroachment, because it has been too fortuitous. The account would be more equal if this were planned, or directed. Their plan, as will be seen later, depends on a wage plan, for the creation of a "parity" from which other results will follow. I think that this part of the Report is still in the region of predisposition.

In a return to history, they say that, because of the war " agriculture is once again playing a vital part in the national effort ". The plough has " come into its own ". The countryside " has assumed an air of busy thriving prosperity ". The reader will consider what is meant by " its own ". The sword has come into its own. It is surely not the essential significance of agriculture to be a war industry. What is the relation to national life of an industry which is represented as depending on war to " put aside the bedraggled condition which in many parts cloaked its beauty " ? Revival of activity is a feature of war, but not in the sense of the phrase used by the Committee. The Committee does not really mean what such a phrase implies, and they grant that the " maintenance " of agriculture cannot mean a wartime condition, but there are interests which tend to such a view, and which would carry the defence aspect of agriculture very far. In particular, the observation of the Committee that in this war the Minister of Agriculture has a right of prior consultation before any land is taken for another purpose will be taken as some justification of their more permanent proposal to that effect. This view of the industry's " own " conditions recurs when the Committee fears that the pre-war trend will reassert itself unless measures are taken against it, which will enable the industry to " fight its battle " on terms of greater equality. I do not wish to make too much of this, but it has a tendency. Since the battle of agriculture has been mainly against foreign supplies, I am afraid of a terminology which may be taken to equate the " own " of agriculture with the " parity " which the Committee wishes to have realised. The unprecedented services rendered by agriculture in this war, and the freedom with which promises have been made to it, will cause the interpretation of its " maintenance " to be a crux of reconstruction.

I do not think that the reference to the preservation of rural amenities means that building as such is always to be regarded as encroachment on the beauty of the countryside. The Committee agrees that in some cases, such as the great reservoirs, development has enhanced beauty. Mr. Dennison argues that it must come to a question of degree, how much beauty we can keep at what other cost. But there is more than that, because the early chapters of this Report are so much of the nature of a predisposition, in which the natural beauty of the countryside is emphasised. An economist will be accused of the vices of his professional nature if he canvasses at all the eloquent quotations from Mr. Wells and the Master of Trinity with which the Report opens. I am not insensitive to the beauty of countrysides, having seen them in great variety all over the world ; but I am also at low values of that important social index, the co-efficient of crural elasticity. My meaning may be expressed by saying that, as applied to the countryside, the word beauty is an ordinary word, not a special word. It means open air and space and skyscape, and change from the town. In its more particular and æsthetic sense, as rural dwellers know, beauty belongs to certain scenes and places, often far from the competition of development. Thus, when the Committee is describing the beauty of the countryside, I feel that they are just describing the countryside as it is : " Its chief characteristic is its attractive patchwork appearance, with an infinite variety of small odd-shaped fields of brown plough-land or green pasture bounded by twisting hedges, narrow winding lanes, small woodlands and copses and isolated trees and hedgerow timber ; quiet streams and placid rivers ; the whole giving place in remoter places to open moorlands and rolling downs. Small groups consisting of a farmhouse with its buildings, and perhaps a cottage or two, sometimes huddled under the lee of a hill, sometimes clustered in the shelter of a small wood, are scattered over the landscape, often at some distance from the nearest village. The villages themselves with their churches, their inns, their cottages, and probably a garage or a petrol pump, perhaps grouped round a green, perhaps straggling along a main road, are dotted over the countryside in large numbers ". I do not think that being brown or green or twisted or straggling is being beautiful, but just being what these things are ; sad poets have moaned in sonnets over the unbearable greenness of grass. I think that, to the townsman, and the above is very much of a picture postcard view for townsmen, the country as a whole is called beautiful in the sense in which anyone to whom we write a letter is " dear ". He is a fellow-creature, just as a barrister is any other barrister's " learned " friend. But if we begin a letter " dear Nancy ", we are specialising, and dearness carries a less conventional meaning. We do not want development to be always liable to a suggestion of encroachment on the " beautiful-country ". Much of the countryside, considered as land, is ordinary or even dull, but it is arched with skyscape which no building can destroy. I agree, too, with the Committee that much of the beauty of cultivated land is connected with its use, and has been made rather than given. There are varied forms of cultivation which in this way derive a conventional beauty. Thus, the paddy fields of Burma or Java are stepped up the sides of the hills in brimming terraces, not essentially beautiful, but only by the suggestion of their use ; so with the wheat-growing prairies or the peasant cultures of Belgium. The Committee point out, too, that in the absence of cultivation the land would revert to jungle ; but the agricultural decline has not meant this, even against the time when we were some 80 per cent. agricultural. I share the Committee's feeling for the splendid beauty of our finest landscapes, but do not want the argument to be tilted against development when the meaning of beauty is as ordinary as " my dear sir ". The Committee do indeed agree that too close a view shows less real beauty than when the landscape is looked at " from afar ".

I think, too, that the general requirements of our whole national development tend to be obscured when the Committee refers to the countryside as " the heritage of the English-speaking world ". Many things might be so described. Some would say that the Bible, or

Shakespeare, was our special heritage. In a well-known poem, our heritage is said to be not the land but the sea. We have no doubt many heritages, but if we are to speak in the singular, the heritage of the English race is just the English race. Every generation has the duty of improving for its children the standard of life, and for that the use of its land must be considered in every respect.

In another aspect, the Committee says a good deal about the difference between urban and rural mentalities. " There remained at the end of the war a degree of misunderstanding even of antagonism between countrymen and townsmen ". " Urban and rural mentalities seem to have had little in common." " As a result of his attitude the urban worker is not always assimilated into country life. He is not, however, the only one to blame. The rural worker is apt to regard all newcomers with some suspicion. He tends to resent the intrusion of outsiders." " The leisurely pace of country life has been rudely disturbed." I heard a good deal on the Tribunal about the advantage of maintaining the countryside for the sake of the " balance of national life " which was maintained by " slow-thinking men ", but I do not think this is good, and the Committee rightly hope for a " gradual fusion of social life ". But they wish also to preserve the country against " rude " disturbance so far as to make the villages a sort of buffer between town and country, by keeping out of them other industries than rural crafts ; I doubt this as a general rule, and certainly not for the purpose of preserving any rural mentality. This country already has too many home fronts on which we are aligned against each other, and I am for every chance of reducing them.

This brings me to the Committee's planning proposals. Their purpose is to prevent fortuitous development ; and the nature of the planning machinery is similar to that proposed by the Uthwatt Committee : a non-Departmental Minister, in order that he may be neutral between Departments concerned with the use of land, who is to be responsible for policy ; a central planning Commission, to act under his directions and translate the policy into concrete terms, and especially to pass upon the plans submitted by local Authorities ; a regional organisation under officials appointed by the Commission, to operate between it and the compulsory local Authorities. It is plain that this can result in one of the greatest Departments of State, as is seen by its alternative title of " Ministry of National Organisation ".

This is a most significant end of a process of argument. There is no meaning in " leaving things to settle themselves ", for it is persons who settle things, or in saying that " things are in the saddle and ride mankind ". Persons can act in more or less organised relation to each other. The minimum meaning of planning is that a course of affairs is enquired into and decisions come to as the result of evidence. That decision might be against any public organisation of private actions, and in that sense free trade, for instance, was a considered plan in this country for a long time. The opposite of any planning is not making any decisions or enquiries at all. The attitude of mind which opposes enquiry and decision in any field, because of some general presumption, is the most legitimate, though quite unhistorical, meaning of *laissez faire*. There would not be organised planning in any field in which the result of enquiry was that it was unnecessary or would do more harm than good; we must distinguish this sense of *laissez faire* from the other. The conclusion of three public enquiries is, that land is a sphere appropriate to organised planning. Of this, there are again certain degrees. You can come to certain conclusions and enact them. This is often the result of Commissions of enquiry; the Commission is then itself the planning body. Or you can set up a planning body, and leave the policy in its hands, having shown reason that this is required; this is free planning. Or you can set up such an Authority, which has powers only within the limits of a policy prescribed to it in some detail; this is tied planning, which is what this case appears to be. I use this phrase without any reflection upon it.

The Committee recognises that the function of the planning body shall be " continuous national planning, and not the formulation of a single fixed plan, for it is essential that planning should be flexible and capable of modification to meet changing needs ". The Commission is to formulate principles, subject to the directions of the Minister as to national policy, and to approve or disapprove local planning in the light of that direction of policy. Looking at the whole organisation, therefore, including the Minister, it would seem that they advise free planning. But there are important qualifications of this freedom. The Minister of Agriculture is to have an exceptional Departmental influence. Every local planning scheme is to be agreed by him before approval by the central planning Authority, which includes the Minister, the Advisory Commission, and the Departments concerned. And, in the second place, the Scott Committee itself lays down recommendations many of which are of the nature of definite directions to the planning Authority.

The veto of the Minister of Agriculture reflects a clear bias of the Committee. There are several statements in which they recognise that their terms of reference are not those of a Committee on agriculture, but on the use of land as a whole. Thus: " both aims (the interests of industry and of the countryside) will be best achieved by measures designed to encourage the growth of industry in those areas where the greatest balance of advantage will result both to the prosperity of industry and to the nation as a whole "; and " it will be the function of the Commission so to plan the whole country that the optimum use is found for every type of land "; or again, " the problem which has to be solved in any instance is whether or not it would be in the national interest to change the existing use of a particular piece of land. The considerations which must go to the making of this decision must necessarily be many and varied, as, for instance, economic, geographic, industrial, agricultural, æsthetic, strategic, social and even international.

Such considerations are matter of national policy ". And there is their more particular admission that " it is often the best agricultural land which is most suitable and least expensive from the building point of view ". Mr. Dennison has no difficulty in showing that a priority of claim by any one interest is not consistent with the observation of the national interest ; and that a flat proposal to throw an " onus of proof " on those who wish to use good agricultural land for building is a contradiction of the terms of reference and, it may be added, of the Committee's own admissions. In this proposal, the Committee simply " fell " for agriculture, whose bright eyes often have this effect. On a reference of a more specific kind concerning agriculture, their proposal might have been arguable, although, as I have said later, the magnitude of the question of transfer between it and building would not then have shown this to be the major problem of the defence of agricultural output and employment.

Apart from this priority, the Committee also makes suggestions of its own which are so important to its point of view as really to amount to an instruction to the planning Authority. Mobile industries, when brought into rural areas, are to be kept out of the villages, and located in the small towns ; the villages thus become a sort of buffer between the rural and urban workers. Before new towns are established in country areas, vacant or derelict sites in existing towns should be fully utilised. A responsibility for restoring the surface should be placed on those who extract minerals, and every lease to prospect or develop minerals should be subject to a deposit or security to cover the cost of restoration. The sporadic building of houses in country areas should be controlled so as to put an onus of proof on new settlers why they should not come into the villages and small towns. Apart from any question of their merits, these proposals are serious limits to the discretion of the planning Authority. It does not seem possible to have it both ways ; to have a national plan which is a " continuous and flexible application of government policy ", and also to have the Royal Commission method of determining that policy in considerable detail. Two members of the Committee have rightly entered a reservation against this two-sided result. One can see that a responsible planning Authority will have great difficulty in accepting the prior right of the Ministry of Agriculture, or in refusing an economic proposal to place an industrial establishment in a village. As regards the restriction on prospecting, I heartily agree that there is a vast amount of cleaning up to do, in some areas especially, but just because there is so much the regional planning bodies require to have a special policy about it at the general expense and, while controlling new development, not particularly to fine the new exploitations. It is for the community to meet necessary costs, and the tips and slag-heaps are an unfortunate necessity. I would doubt the instruction, interesting as it is, about the control of sporadic building. It is the spread of cottage building over the land which in many countries, like Denmark, makes the countryside pleasant and homely ; this is a matter of taste in building and layout, on which a planning body is likely to want much freedom.

Beyond these instructions to the planning Authority, the Committee has in effect given important instructions to the government in respect of agricultural policy. I think that in this they go much beyond their reference. The government is to formulate a long-term policy for agriculture, and this is to be such as will, first, maintain " the traditional mixed character of the industry", and, second, place agricultural wages on a parity with other wages. By this parity they mean that the villager will be able to pay economic rents for houses, and the charges for other services, as good as those which workers of similar grade can get elsewhere. This is to be so done as to stop the drift to the cities, so that the size of the industry is to be maintained. They would, they say, have been sent on a fool's errand if " a decadent agriculture was to be maintained ", so that their reference must imply a " prosperous and progressive agriculture " ; and on this they place the further gloss that it must mean the "continuance and revival of the traditional mixed character of British agriculture ". Now it is quite possible that the government will decide to maintain agriculture at some cost to the country, after the war ; this is one of the general problems of reconstruction. But it is necessary to realise what this will mean to such an economy as ours, and this Committee rather prejudices its own proposals for planning by attaching them to a particular policy, which is to be the " condition precedent " to these proposals. Suppose the planning body says, well, these proposals depend on an essential condition, and that condition is not given, then the valuable work of the Committee is all in the air. It is quite likely that the condition cannot be realised for an agriculture that is both large and prosperous. There is nothing decadent except arable in a change to grass farming, and the arable question must depend on the conditions of the peace. But there is much to consider regarding amenity and the interests of the land which is independent of this, and that was the function of this Committee. They had to consider how the Barlow proposals for dispersal could cause least interference with the countryside. The result of their digression into agricultural policy has been to start Mr. Dennison on reminding them of what has been so often said and discussed before agricultural Committees, Commissions, and Tribunals. Having heard it all on the Tribunal, I think his argument is unanswerable, though I adhere to the reservation I made then, that national defence is an over-riding consideration. A high cost may have to be incurred, and I do not know that storage of imports is an answer to the policy of cultivation ; in our conditions of climate and rats, allowing for the great visibility of elevators and our nearness to the Continent, we may have to incur the cost. I do not know, but I could nevertheless not accept that proposals on the reference of this Committee should make a particular agricultural policy a condition precedent to their validity. Agriculture may not be by any means the industry most difficult to maintain, in terms of employment or prosperity, after this war ; and we should not prejudge that complicated situation. While appearing to throw a responsibility on the government, the Committee have in fact made a very large proposal, without arguing it, and without really helping their other proposals to find acceptance.

The land gives a group, but a limited group, of occupations. There are crops and animals. The list is the same in each case as it has always been. We cannot make a new kind of animal or grow a new kind of crop. Contrast this with the immense diversity of manufactured products, the list of which is always being increased. If an urban industry is depressed, there can be a drift to other industries, without the worker ceasing to be urban. But this can far less happen to the agriculturist, without his ceasing to be rural. Apart from other causes of the drift, this one is fundamental ; and few if any industrialised countries have prevented the drift, even under protection or subsidy. It has been thought to be good for the health of cities that there should be some such movement, but this is a lessening consideration. I do not go as far as Mr. Dennison, who thinks it should rather be assisted than retarded, and on the Tribunal I suggested what should be taken as danger point. But that is far from agreeing that a particular method of farming or size of the industry should be made a condition precedent to any schemes of amenity or control of building in rural areas. It is not a fools' errand on which the Committee have gone in many of their proposals for regulation, which are in themselves acceptable.

It is not unfair to say more than this concerning the " conditions precedent ". The Committee says that " the evidence has made it clear that those effects of constructional development in country areas which are harmful to agriculture and the countryside generally, cannot be nullified unless the conditions of life of the rural community are considerably improved and placed on a parity with those of urban workers ". And later : " We wish to stress that these steps must be considered as *conditions precedent* to the introduction of industry in country areas if agriculture and the countryside are not to suffer harm ". Now a Committee which has given so much space to writing up and idealising the country against the town, and has glossed its reference as above stated in support of this, comes near to placing its concessions to rural building in the guise of a threat and of challenging the government to reject them, unless it is willing to support a particular type of agriculture on conditions which are certainly controversial, and the reactions of which are not considered. Is that the way to closure the argument on agricultural policy ? I do not suggest that the Committee was aware of this aspect of their Report as a whole, but it is an interpretation which can without much strain be placed upon it.

What is the nature and, still more, the size of the dispute between the building and other uses of the land ? The reference shows no bias between building and agriculture, both of which are important industries, but we can start from either end. If we choose, as the Committee does by its opening chapters, to start from the agricultural end, then the tendency to magnify that side begins. When the Coal Commission was set to enquire into that industry, it had no hesitation in observing that " the coal-mining industry occupies a unique and exceptional place in our national life, and there is no other industry comparable with it ". I imagine that many industries, including building, could stake out a claim for uniqueness and national importance. This Committee begins with the indubitable assertion that " land is one of the prime essentials of human existence ". Its proper and profitable use is therefore of basic importance to every nation, and here everyone still goes with them. " It is particularly so where the population is large in relation to the amount of land available to support that population ", and England is the most densely populated of all great countries. But the land available to support our population is not only our own land, unless support means standing room. " All the materials on which mankind depends" come from land, but mainly in our case from foreign land. The best use of our own land is subject to the consideration of all our resources for food supply and other forms of production. The density of our population is a reason for emphasising the important difference that, while we can use foreign land to supply us, we cannot use it to build on. The necessity for dispersal which the Barlow Report perceived is alleviated by this removal of pressure on the alternative use of land, and it was here again left to Mr. Dennison to deflate an excessive terminology. The amount, and still more the percentage, of our land which may be in dispute between farming and building would, because of this consideration, have to be large in order to be significant from any point of view except amenity.

Now, how much is really in dispute ? The Committee have given an interesting table showing the competition for land in England and Wales. Before the war, more than 80 per cent. of the whole area was in agricultural production of some sort, and another 5·5 per cent. was forest land. All buildings and other constructional developments required about 11 per cent. This includes buildings needed for agricultural purposes. In order to estimate the annual encroachment of building, still including agricultural needs, I have had slightly to revise the Committee's figures of area. The 58,340 square miles which they quote includes land and water which is outside the problem of this competition. At the mid-point of the twelve pre-war years for which they give figures, the whole agricultural area, including rough grazings, was 47,585 square miles, and about 6,500 square miles were under development of some kind. Against this total of some 54,000 square miles, the net increase of development is given as 60,000 acres, or 94 square miles per annum. This makes 0·17 per cent. of the whole area in question, or about 0·2 per cent. of encroachment or transfer, twenty square miles in every ten thousand, or the annual transfer of a square of land with a side of 9·7 miles. In twelve years the side of this square is increased to about 33 miles. This was on the whole an active period of building, and included in the latter years a rapid growth of military construction. Of course, we cannot go on multiplying by any number of years, and the non-military development was falling in these latter years. Apart from military needs, the transfer was about 0·18 per cent. annually. (Mr. Dennison gives 0·15 per cent. of the area as the Committee took it.) The Committee point out that land is sometimes held out of use in the hope of development, and this should be set off against the fact that much building is for agricultural use. They agree, however, that extractive industries have no choice of location, that public utilities may have to be located in the country, and that the case of some "rooted" or heavy industries is doubtful. We are, therefore, reduced to relatively small figures of encroachment, and to still smaller ones of competition for agriculturally "good" land, which may also be "good" building land. Is the dispute about the "onus of proof" an argument *de minimis* ? One can easily imagine a case of some ten acres leading to a hectic discussion in the Departments, and it would be interesting to see the papers, in which the *δικᾶιος λόγος* and the *ἄδικος λόγος* would extend themselves and each other. How does the magnitude of these figures bear on the excursion into agricultural policy ? Mr. Dennison's comment is curt, and is not taken by his colleagues.

The proposal to extend electricity, gas, and water by free installations to the villages and cottages is as expensive as it is desirable, and anyone who has lived on a farm can realise what the absence of such services means to the life of the farmer's wife. How far a planning body can carry it will depend on the policy by which agriculture is "maintained" in this way that, if it is thought necessary to have a large agriculture, the conditions of peace will be still dangerous, and there will also be some measure of conscription, and of large munitions industries. The idea that we are going to be very well off after, or even as a result of the war, should be treated without any respect. The more expensive our requirements for safety, the more burdensome this proposal, but in these conditions we may have to try to afford it. The safer we are after the war, the more affordable it will be, without its capital cost being much less.

The Committee makes many admirable suggestions for the protection of amenities and the revival of village life, and even those proposals from which one may differ are set out with great earnestness ; its literary form, too, will give it a wide support. But I hope that the planning body will give due weight to Mr. Dennison's reply, and will assume a real freedom in applying all that has been called to its attention. In this field, it cannot be so purely executive as for the more technical purposes of the Uthwatt Report. And it cannot assume, or have thrust on it, or thrust on the nation, a particular agricultural policy ; for that, we have still independently to wait.

# The Rôle of Expectations in Economics as a Social Science

### By L. M. Lachmann

## I

In modern theory the introduction of expectations has opened new vistas to the economist and, at the same time, set him a new problem. It has made him realise that economic action concerned with the future, so far from being strictly determined by a set of objective " data ", is often decided upon in a penumbra of doubt and uncertainty, vague hopes and inarticulate fears, in which ultimate decision may well depend on mental alertness, ability to read the signs of a changing world, and readiness to face the unknown. But it has also compelled him to reflect on the causal explanation of expectations, to ask himself why they are what they are. This problem bristles with difficulties.

Given this fact and the natural proclivity of every science to become more limited in scope as it grows more conscious of its premisses, it was perhaps inevitable that economists confronted with this problem should have attempted to dispose of it by relegating expectations to the category of " data " alongside with wants, resources, and the technical facts of production. This line was in fact taken by Lord Keynes,[1] Dr. Morgenstern,[2] Professor Myrdal,[3] and Dr. Rosenstein-Rodan.[4] But it is readily seen that expectations must feel ill at ease in this company. What entitles us to treat wants and resources as data and disinterest ourselves in their causal derivation is the simple fact that *qua economists* we have nothing to say about them. Why the geographical distribution of mineral resources is what it is, why the cinema-going public of the 1930's preferred moving pictures directed by René Clair to moving pictures directed by Ernst Lubitsch, are in themselves interesting questions, but the economist has no answer to them. Expectations, on the other hand, are on a somewhat different plane as they are, while wants and resources are not, largely the result of the experience of economic processes. It is therefore hardly surprising that the treatment of expectations as data the explanation of which is not the task of the economist, should have given rise to strong protests. Outstanding among the critics are Dr. Lundberg and Professor Schumpeter.

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[1] At least for the " state of long-term expectation ", *General Theory*, pp. 147–49.

[2] " Vollkommene Voraussicht und wirtschaftliches Gleichgewicht." *Zeitschrift Für Nationalökonomie*, September, 1935.

[3] *Monetary Equilibrium*, London, 1939.

[4] " The Coordination of the General Theories of Money and Price," Economica, August, 1936.

"It is sensible to link actions with expectations," states Dr. Lundberg, "only if the latter can be explained on the basis of past and present economic events. Total lack of correlation here would mean the complete liquidation of economics as a science. Not even an assumption of certain anticipations as given and an analysis of consequent plans and actions on the basis just mentioned would have the slightest interest . . . In every process of economic reasoning we therefore have to make certain assumptions, often not specified, concerning the relations between expectations on the one hand and current or past prices, profits, etc., on the other."[1]

"Expectations cannot be used as part of our ultimate data in the same way as taste for tobacco can," writes Professor Schumpeter. "Unless we know why people expect what they expect, any argument is completely valueless which appeals to them as *causæ efficientes*. Such appeals enter into the class of pseudo-explanations which already amused Molière."[2] "If we discontinue the practice of treating expectations as if they were ultimate data, and treat them as what they are—variables which it is our task to explain—properly linking them up with the business situations that give rise to them, we shall succeed in restricting expectations to those which we actually observe and not only reduce their influence to its proper proportions but also understand how the course of events moulds them and at certain times so turns them as to make them work toward equilibrium."[3]

Unfortunately, however much we may agree with the point of view of these authors, it is not easy to carry out their proposals which are by no means unambiguous. In order to link up expectations "with the business situations that give rise to them" we must first of all define a "business situation". If we define it in objective terms (as a combination of prices paid, quantities produced and sold, etc.) we soon find that the relationship between business situations and expectations is not uniquely determined as the same "business situation" may give rise to various kinds of expectations. A price rise, for instance, may lead to expectations either of a future fall, if the people in the market have some kind of "normal level" at the back of their minds, or of a future rise, if inflationary forces are suspected to be at work. If, on the other hand, we define "business situation" in subjective terms, viz. as the interpretation which the people give to the objective facts, there will be as many "business situations" as there are different interpretations of the same facts, and they will all exist alongside each other.

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[1] Erik Lundberg: *Studies in the Theory of Economic Expansion*, Stockholm and London, 1937, p. 175.

[2] J. A. Schumpeter: *Business Cycles*, New York, 1939, Vol. I, p. 140.

[3] *Ibid.*, p. 55.

The absence of a uniform relationship between a set of observable events which might be described as a *situation* on the one hand, and expectations on the other hand, is thus seen to be the crux of the whole matter. Expectations, it is true, are largely a response to events experienced in the past, but the *modus operandi* of the response is not the same in all cases even of the same experience. This experience, before being transformed into expectations, has, so to speak, to pass through a " filter " in the human mind, and the undefinable character of this process makes the outcome of it unpredictable. We provisionally conclude that expectations are the result of a variety of factors only some of which are observable events, and only some of which are of an economic nature. It follows that they have to be regarded as economically indeterminate and cannot be treated as " variables which it is our task to explain ".

Under these circumstances, what can the economist do but construct various hypothetical types of expectations conceived as responses to various hypothetical situations, and then leave the process of selection to empirical verification in the light of economic history? Several such " ideal types " either of expectations, like Lord Keynes' " long-term " and " short-term " expectations, or of the holders of expectations, like Professor Schumpeter's " static producer " and " dynamic entrepreneur " or Professor Hicks' " sensitive " and " insensitive " traders, have already been evolved and served to elucidate important dynamic problems. This is a most promising field of research and much progress can be achieved along this line. It seems to us, however, that it is possible to carry the general theory of expectations a stage farther, and to the demonstration of this possibility the present paper is devoted.

The next step in the study of expectations, to be sure, has to consist in evolving hypothetical " ideal types " and testing them in the light of economic history. But it cannot be emphasised too strongly that if these efforts were to be confined to the study of relations between objective facts and expectations they would be quite useless. The Social World consists not of facts but of our interpretations of the facts. Nothing will be achieved in the way of an inductive study of expectations until people's expectational responses to the facts of a situation are made *intelligible* to us, until we are able to understand *why* the acting and expecting individuals interpreted a set of facts in the way they actually did. From this point of view we need not deplore unduly the indeterminateness of expectations, for it is *intelligibility* and not *determinateness* that social science should strive to achieve.

We have now reached a point at which it must be evident that we are here facing a fundamental issue in the methodology of economics, and of social science in general. The intricacy of our problem is derived from the inadequacy of the traditional methods of analysis in a case of indeterminateness. Before we can pursue our study of expectations any further we shall have to reconsider some of the first principles of economic analysis.

## II

All human action is directed towards purposes. Hence, as Professor Knight has repeatedly reminded us in recent years, all human activity is problem-solving. Man, before setting out on his course of action, has to make a plan embodying the means at his disposal and the obstacles he is likely to encounter, otherwise his action is not (rational) conduct but (non-rational) mere behaviour. Before starting on his way he tries to chart the path leading to the achievement of his purpose in the topography of his mind. If we say that we wish to " explain " an action, what we mean is not merely that we wish to know its purpose, but also that we wish to see the plan behind the action. Plan, a product of the mind, is both the common denominator of all human action and its mental pattern, and it is by reducing " action " to " plan " that we " understand " the actions of individuals. Plan is the *tertium conparationis* between our mind and the mind of the person who acts.

In economic action the problem to be solved is to devise a plan for the allocation of scarce resources to alternative wants in such a way as to maximize satisfaction. Equilibrium theory, which studies the problem and its implications, teaches us that, for each individual at least, the problem has a determinate solution. And since the elements of the plan are quantifiable, if not measurable, the problem and its solution can be illustrated *more mathematico.* However, that a problem has a determinate solution does not entail that those attempting its solution will actually succeed, otherwise there would be no failures in examinations or in business. A plan may fail, of course, for almost any number of reasons. For instance, it may have been faulty from the beginning because of lack of consistency between its various elements; or, while it was perfectly consistent, unexpected obstacles may have been encountered in the course of its execution of which it had failed to take account; or the planner may have misjudged the extent and efficiency of the resources at his disposal. It will be noted that in the second and third instance, but not in the first, failure is due to wrong expectations. Expectations therefore take a prominent place in the theory of economic action; but thus far such a theory does not exist.

It has to be admitted that hitherto the scope of economic theory has been unduly restricted to the formal characteristics of the economic problem and its implications. Equilibrium economics (what Professor Hayek has termed " The Pure Logic of Choice ") studies the full implications of a set of data, the " conditions of equilibrium "; it does not study the ways in which these logical implications are translated into human action, which is thus conceived as a quasi-automatic response to an external " stimulus ". But in the theory of economic action no such mechanistic preconception is admissible, a point which the introduction of expectations brings out with all necessary clarity. Unfortunately, the Pure Logic of Choice has filled the minds of economists to such an extent that the study of the actual means and ways by which men try to realise their aims has come to be sadly neglected.[1] Economists, not unnaturally, prefer to do their field-work in a pleasant green valley where the population register is exhaustive and everybody known to live on either the right or the left side of an equation. Only on rare occasions—and scarcely ever of their own free will—do they embark on excursions into the rough uplands of the World of Change to chart the country and to record the folkways of its savage inhabitants; whence they return with grim tales of horror and frustration. Traces of such folklore can be found in the touching Swedish saga of the unhappy partnership of Ex Ante (the plan) and Ex Post (the outcome of action).

Needless to say, if our attention is thus confined to the formal characteristics of the economic problem, if our approach remains " functional " rather than " causal-genetic ", we shall not only be unable to find explanations for failure to solve the problem, but also be in no way equipped to deal with characteristic instances of failure, like crises and misinvestment; hence, the peculiar helplessness of equilibrium theory in front of trade cycle problems. Whenever confronted with such problems, we shall almost inevitably be biased in favour of an explanation which runs in terms of initial inconsistency of, at least some, plans, for consistency is precisely one of those formal characteristics which we are best trained to investigate. A typical example of this is the explanation of industrial fluctuations which is currently in fashion. Such fluctuations are regarded solely as variations in the degree of utilisation of the resources of Society, and under-utilisation is explained by inconsistency between the plans of investment planners and of saver-consumers. We cannot therefore be surprised to learn that such theories have no real explanation for malinvestment and capital losses in investment goods industries, and that one of their favourite assumptions is that all such goods (tin, copper !) are made " to order ! "

In the light of these considerations we are now able to view the indeterminateness of expectations in its proper perspective. In a world of imperfect foresight in which no plan can meet all contingencies all human activity is bound to be indeterminate; in this respect expectations are simply on a par with everything else. What may (and in the case of economic activity happens to) be determinate is the problem which this activity seeks to solve, but it does not follow that in this it will succeed; there is, after all, a difference between a problem tackled and a problem solved. Determinateness, we realise, is a possible property of problems; it is not a possible property of human action.

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[1] Fortunately not without exception. While, for instance, equilibrium theory takes the shape of production functions for granted, Professor Schumpeter's theory of Innovation, which is really a theory of entrepreneurial action, explains how in the course of economic development one production function comes to supersede another.

The reader will not, we hope, infer from all this that the Pure Logic of Choice with its equations and its indifference curves is altogether useless. On the contrary, it serves a most useful purpose by making economic activity *intelligible* to our problem-solving mind. For it is only by reducing the apparently chaotic World of Action to a mental pattern of relative simplicity that our problem-solving mind can comprehend it. All we have to remember is that to describe an action in terms of a problem is, of course, not to say that it will succeed in solving it.

After this long digression we may now return to our study of expectations. It is evident that if so often we fail to solve our problems, in a world of imperfect foresight the chief reason has to be sought in our being misguided by wrong expectations. More particularly will this be so in the economic field, in which the theoretical interest in expectations arose, not by accident, from the study of crises and depressions, the classical instances of failure to solve the problem of the optimum allocation of resources. That expectations are germane to failure is plain enough, but what precisely is the character of their relationship?

We have seen that we need not deplore the indeterminateness of expectations because this quality they share with all other forms of human activity. But, we said, it is the task of social science to make them intelligible. To make an action intelligible means to show not only its purpose, but also the general design of the plan behind it. What, then, are expectations? We saw that all human conduct (as distinct from mere behaviour) presupposes a plan. We now have to realise that, as a prerequisite to making a plan, we have to draw a mental picture of the situation in which we are going to act, and that the formation of expectations is incidental to the drawing of this picture. Such a picture of the situation will be drawn differently by different individuals confronted with the same observable events in accordance with psychic differences such as temperament, but the degree of variation between them does not entirely depend on psychic factors. In a stationary world in which the same observable events continually recur this degree of variation would be small although, owing to the psychic factors, it probably never would reach zero. But in a World in Motion it must be large, chiefly among other reasons because here every view of a situation necessarily implies a judgment on the character of the forces producing and governing motion. Two farmers confronted with the same observable event, a rise in apple prices, will yet take different views of the situation and react differently if one interprets it as a symptom of inflation and the other as indicating a shift in demand under the influence of vegetarianism.

The upshot of all this is, of course, the familiar proposition that observable events as such have no significance except with reference to a framework of interpretation which is logically prior to them. From this there follow two conclusions, a narrower one concerning expectations, and a broader one pertaining to the formulation of the economic problem in a dynamic World.

As to the first, our argument appears to shed some light on the nature of the " filter " which, as we learned, forms the link between observable events and expectations. We now know that, while it remains true that our expectations for the future are a response to our experience of the past, the mode of our response is largely governed by our interpretation of this experience. In a World of Change this interpretation is bound to reflect strongly what we believe to be the major forces operating in this World, causing and governing change. We now realise that ultimately it is the *subjective* nature of these beliefs which imparts indeterminateness to expectations as it is their *mental* nature which renders them capable of explanation.

Our second conclusion points to the desirability of a dynamic revision of the formulation of the economic problem. The problem is usually stated in terms of (objective) " resources " and (subjective) " wants ". In a stationary World these terms may have an unambiguous meaning, but in a dynamic World what is a resource depends on expectations, and so does what constitutes a want worth satisfying. In a properly dynamic formulation of the economic problem all elements have to be subjective, but there are two layers of subjectivism, rooted in different spheres of the mind, which must not be confused, viz. the subjectivism of want and the subjectivism of interpretation.

## III

We have now reached a point at which we may pause and look around for an opportunity to test the efficacy of our newly forged analytical tools. Ultimately, of course, the only satisfactory test of any theoretical construction is the light it sheds on some segment of reality, its making an otherwise incomprehensible set of facts intelligible to us. Such a test will be applied in the concluding section, but prior to it we shall embark on another one of a somewhat different nature. In the light of the knowledge thus far gained we shall examine Professor Hicks' concept of " elasticity of expectations ". Considering the prominent place which this notion has come to occupy in the analytical apparatus of up-to-date economic theory, it should provide us with a suitable starting-point from which to measure whatever further progress it may be possible to make by the help of other devices. The purpose of our test is to see whether the lamp we have constructed is capable of throwing light on corners of the problem of expectations which the lighting apparatus hitherto in use had left dark.

In dealing with expectations Professor Hicks wisely refrains from seeking determinateness. He distinguishes between the influence of current prices on expectations and, on the other hand, all other influences the effects of which are labelled " autonomous changes ". Neglecting the latter he concentrates on the former type of influence. But seeing that " that influence may have various degrees of intensity, and work in various different ways "[1] we realise that we need a criterion of classification, and it is for this rôle that the "elasticity of expectations " is cast.[2] Its great merit is that by making it unnecessary to postulate a once-and-for-all uniform relationship between changes in current prices and expectations it enables us to deal with variable forms of this relationship. Its defect, we believe, is that, being a measure, it cannot tell us *why* this relation should take these variable forms any more than the most elaborate thermometer can tell us the causes of the fever from which the patient is suffering. However, for the greater part of his study of dynamic equilibrium Professor Hicks is content to make less than full use of the potentialities of his weapon and to assume the elasticity to be unity; he is in fact assuming a uniform relationship, and as long as he does this the defect mentioned does not cause much harm. But as soon as, in his discussion of wage rigidity, he abandons this restrictive assumption and allows for variations in this relationship, he apparently becomes conscious of the defect and feels compelled to give some kind of causal explanation of the forms which these variations may take. Unfortunately, however, the study of these variations is immediately restricted to those existing simultaneously between different groups of persons, after which it is not surprising that the causal explanation runs in terms of a spurious brand of " group psychology ". It is sought in the greater or smaller " sensitivity " with which different people react to identical present changes.[3]

It is not easy to attach any precise meaning to these terms. Does Professor Hicks seriously maintain that the same individual confronted with the same kind of change will invariably react in an identical—and incidentally, predictable—manner? Only such invariability of reaction would entitle us to use intensity of reaction as a criterion of classification. If, on the other hand, we allow for changes over time in the " sensitivity " of individuals, and thus for changes in the composition of the two groups, is it not precisely our task to explain these changes? Since there can be little doubt that in fact men's expectational reactions to change are subject to wide fluctuations, we have to find a principle informing us in accordance with what these reactions vary. There may be numerous reasons, but it would seem that they can all be reduced to the simple fact that men's reactions to identical observable events will vary if for any reason these come to have a different meaning to them. The conclusion suggests itself that whether a given price change—or, for that matter, any other observable event—will at different times give rise to identical expectations will largely depend on the way in which people interpret it. Interpreting an event means to fit it into a picture of the " situation ", a concept of a structure which serves as framework of reference. It follows that the " elasticity of expectations ", if it is not to lead us into having to accept absurdities like an invariant " sensitivity ", itself requires interpretation in the light of our argument.

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[1] J. R. Hicks: *Value and Capital*, 1939, p. 205.

[2] " I define the elasticity of a particular person's expectations of the price of commodity *X* as the ratio of the proportional rise in expected future prices of *X* to the proportional rise in its current price." *Ibid.*, p. 205.

[3] " Some people's expectations do usually seem to be in fact fairly steady; they do not easily lose confidence in the maintenance of a steady level in the prices with which they are concerned; so that, when these prices vary, their natural interpretation of the situation is that the current price has become abnormally low, or abnormally high. But there are other people whose expectations are much more sensitive, who easily persuade themselves that any change in prices which they experience is a permanent change, or even that prices will go on changing in the same direction." *Ibid.*, p. 271.

## IV

By now the reader will probably have grown impatient to see our theory of economic action go into action. We said above that ultimately the only satisfactory test of a theoretical construction is its capability of throwing light on what otherwise must remain dark corners of reality. We now propose to test the method we advocate by showing its usefulness in elucidating a problem which has loomed large in recent controversies on the rate of interest ; we shall thus be concerned with interest-expectations, not with price-expectations. But before testing it let us briefly summarise the position we defend.

All human action is directed towards purposes and therefore requires a plan. Plans are not made *in vacuo*, and the planner has therefore to draw a mental picture of the situation in which he will have to act, of the constellation of circumstances which he cannot, or at least thinks he cannot, change and which to him are " data ". We assert that the formation of expectations is incidental to, and derives its meaning from, this activity of the mind, and we therefore conclude that expectations have to be interpreted with reference to the situation as a whole *as the expecting individual sees it.*

The problem we shall discuss refers to the influence of expectations on the rate of interest. It is not our intention to take part in any of the numerous and fierce controversies which have of late raged on the theory of interest. On the contrary, we take as our starting-point a proposition which we believe to be entirely beyond controversy : that interest-expectations are one of the factors influencing the rate of interest. It is perhaps not unprofitable to insert here a brief sketch of the evolution of this idea in modern economic thought. It was introduced by Lord Keynes who used it as the main pillar to give shape and concreteness to his liquidity preference curve the negative slope of which could not otherwise be convincingly demonstrated.[1]

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[1] *General Theory*, pp. 168–72.

It was further elucidated by Mr. Durbin, who pointed out that " Large stocks of securities, just as much as large stocks of commodities, constitute a continuous and serious threat to monetary equilibrium, and the existence of stocks of securities is inevitable in a way that stocks of commodities are not ",[1] for the larger stocks are relatively to current output the wider the scope for speculative price fluctuations and the smaller the influence of long-run supply on market price. Mr. Harrod drew the important practical conclusion that in a capital market which has a very definite expectation about the future rate of interest " it seems improbable that banking policy, however inspired and well informed, could secure a sufficient fluctuation in long-term interest rates to ensure a steady advance ",[2] as at rates lower than the expected the supply of securities would tend to become almost infinitely elastic. Professor Hicks does " not believe that we can count upon anything more than a small elasticity of interest-expectations ".[3] " When the rate of interest (any rate of interest) rises or falls very far, there is a real presumption that it will come back to a " normal " level. This consideration would seem to prevent interest-expectations from being very elastic ".[4] But to the extent to which expectations are inelastic other influences on the current rate will grow correspondingly weaker. In Dr. Thomas Wilson, its latest exponent, the idea that the long-term rate of interest is largely fixed by " speculation " in the capital market has almost assumed the character of a major economic principle and been made the cornerstone of a trade cycle theory.[5]

We do not question the occurrence of inelastic interest-expectations, but it is a corollary of our argument that we have to insist on an *explanation* of such occurrences. It is already a little surprising that while what in common parlance are described as " speculative markets " are mostly characterised by wide and frequent price fluctuations, " speculation " is here held responsible for price inflexibility. But so far this merely goes to show that " speculation " may be a misnomer for the phenomenon under discussion. More significant is that, almost under our hands, the proposition that expectations are *one* of the factors influencing the rate of interest has changed into the proposition that if expectations are inelastic none of the other factors will have a chance to influence the result, because with a highly elastic supply of securities the other factors may influence the volume of sales but cannot affect price. This already suggests that unless the rate of interest is to be " left hanging by its own bootstraps " the other factors must somehow already be taken into account by the expectations. Furthermore, if the case of inelastic interest-expectations has such far-reaching consequences it clearly is our task to investigate what causes such expectations.

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[1] E. F. M. Durbin: *The Problem of Credit Policy*, 1935, p. 103.

[2] R. F. Harrod: *The Trade Cycle*, 1936, pp. 124–5.

[3] J. R. Hicks: op. cit., p. 282.

[4] *Ibid.*, p. 262.

[5] Th. Wilson: *Fluctuations in Income and Employment*," 1942, Ch. II, pp. 16–26.

A little reflection will show that if in a market a strong increase in demand does not lead to any appreciable rise in price, not only must supply be extremely elastic, but where large stocks are the cause of this elasticity, holders of stocks must have a reason for selling out. They clearly will do it only if they have reasons to believe that the present strong demand is not only of an exceptional but of a transitory nature, and that for this very reason price will in the long run not be affected by it. If we apply this reasoning to the capital market we find that interest-expectations are most likely to be inelastic in a situation in which the capital market, that is to say, the majority of holders of securities, does not believe in the permanence of the forces exerting pressure on the market and hopes later on to be able to "re-stock" cheaply. It follows that if we find a case in which increased savings do not cause any appreciable fall in the rate of interest this indicates that the capital market has its suspicions——which may turn out to be entirely unjustified—about the permanent character of this sudden increase in the demand for securities. We are now able to understand the meaning of the "normal level" in the minds of people whose expectations are inelastic: this is a level determined by what are believed to be permanently operative forces. A market will exhibit inelastic expectations only if it believes that price is ultimately governed by long-run forces, and if it has a fairly definite conception of what these forces are. A capital market with inelastic interest-expectations is then a market which refuses to be impressed by present-day demand for securities which it believes to be short-lived. If therefore in a depression we find the long-term rate of interest remaining relatively inflexible this indicates that, rightly or wrongly, the capital market believes in the continued existence of investment opportunities yielding marginal profit at the former level, investment opportunities which the depression may have obscured but which it has not obliterated. For the same reason, in such a case, as Mr. Harrod predicted, an attempt to put the bond market under pressure by means of open-market operations is likely to prove a failure.

Finally, we always have to remember that whenever we observe large transactions taking place at little price change this indicates a case of conflicting expectations. It is scarcely necessary to remind the reader that we are here concerned solely with explaining a certain class of expectations, not with judging them in the light of *ex post* knowledge which the expecting individuals did not possess. It is indeed fairly obvious that in a dynamic economy with rapid technical progress and wide and frequent income fluctuations all expectations based on the prevalence of long-run trends must be of a somewhat problematical character, but to our present problem this is strictly irrelevant.

If inelastic expectations are really as frequent and important as some writers would have us believe, an interesting problem arises with regard to the interpretation of Wicksellian theory, more particularly in its Austrian version. According to this doctrine booms and slumps are engineered by banks lowering the " money rate of interest " below its " natural level ", or raising it above it. Whatever the precise meaning of these terms, we now know that if banks are to succeed in altering the long-term rate of interest, expectations have to be very elastic. Seen from this angle, the Wicksellian theory appears to be based on a very special assumption, viz. of a capital market without a very strong mind of its own, always ready to follow a lead on the spur of the moment, and easily led into mistaking an ephemeral phenomenon for a symptom of a change in the economic structure. Without fairly elastic expectations there can therefore be no crisis of the Austro-Wicksellian type. But again, before we can accept this theory we are entitled to hear an explanation why elastic expectations should be prevalent. Such a gullible capital market we should expect to find in an economy the structure of which is still highly fluid and in which long-run forces have not yet had time to take shape. We tentatively suggest that such a state of expectations may be typical of an economy in the early stages of industrialisation, or of an economy undergoing " rejuvenation " owing to rapid technical progress.

In reality, of course, expectations of greatly varying degrees of elasticity are met with. It may be possible to reconcile apparently irreconcilable theories by reducing their differences to different assumptions about the prevailing type of expectation. But the story does not end here. In a World of Change no one type of expectation can be relied upon to provide stability. Neither a gullible capital market nor an obstinate one, nor, we may add, any intermediate variety is in itself a bulwark against crises of every kind. They each provide us with protection against some afflictions while leaving us unprotected against others. To investigate in what conditions what type of expectations is likely to have a stabilising or destabilising influence is no doubt one of the next tasks of dynamic theory. We submit that it cannot be successfully tackled unless expectations are made the subject of causal explanation.

# The Fortune Teller

By RICHARD STONE

"*You can fool all the people some of the time and some of the people all the time, but you cannot fool all the people all of the time.*"
—Attributed to ABRAHAM LINCOLN.

## I

## INTRODUCTION

THE great interest of the subject matter of Mr. Colin Clark's latest book[1] is obvious. It was bound to attract considerable attention, not only among Mr. Clark's colleagues but also among the many laymen who are acquainted with his pioneer work. This being so, the conscientious reviewer must act on behalf of those readers who are unable to judge for themselves, examine its arguments and pretensions with especial care and attempt an assessment of the scientific value of the method and conclusions. For this reason the present reviewer has set out in an orderly fashion what he takes to be the economic argument, and has then proceeded to a critical examination both of it and of the statistical basis on which it rests.

The general conclusion that emerges from this examination is that the sole merit of the book is that its subject matter is interesting. It would not have called for so extended a notice but for the world-wide reputation of its author and the rhapsodic chorus of indolent reviewers in the popular press. It is regrettable that such a celebrated writer can bring himself to publish so misleading and inadequate a study. Nor is it a service to truth or enlightenment that such conclusions should be transmitted to the general public by popular reviewers whose candour compels them to admit that they have been unable to follow the arguments.

## II

## THE CONTENTS OF THE BOOK

The purpose of this book, says the author in his first chapter, is not to prophesy but to describe "what state of economic affairs might reasonably be expected in 1960". There are many possibilities, we are told, which may make the actual differ from expectation, but Mr. Clark argues against the view that this sort of estimation is impracticable, either because economic changes are violent or because they depend so much on political and social factors. The latter argument he rejects as definitely wrong. No attempt is made to justify this fundamental assertion, and Mr. Clark suggests, with complete irrelevance, that most people who disagree with him on this important point are intellectually lazy. There is naturally no evidence for this generalisation either.

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[1] *The Economics of 1960.* Macmillan, 1942, pp. 118, 8s. 6d.

On the question of the violence of economic change, Mr. Clark says that it relates mainly to the trade cycle and is of no significance in the study of long period economics. To bear out this view, he refers us to " the following diagram " by which, presumably, he means the second diagram at the end of the book. This shows, for a number of countries over several recent decades, the movements in real income per occupied person in work measured in international units[1] per 2,500 hours' work, i.e., real income per man hour. These series, it is true, move fairly smoothly through time. It would not have been difficult to make their movements smoother still for this could be done by adjusting output per head for the changes in productivity due to variations from normal in the scale of operations. But neither set of figures would do much to support Mr. Clark's contention, for there are a large number of series relevant to economic wellbeing which do vary through time and which are not proof against sudden shocks. It may surprise some readers that in this book, which purports to deal with the real world, there is almost no mention of unemployment, monopoly, the control of industry or public finance, each of which, one might suppose, is capable of having a considerable influence on the course of economic events.

The general outline of the work is this : Chapter II deals with the trend of total and working population by countries. Chapter III is concerned with the trend of secondary and tertiary productivity and the relation between the latter and real income per head. Chapter IV introduces the supply and demand of primary products and sets out three relationships which enter into the subsequent analysis. They are the relations between (*a*) production per head and density of male workers per unit of farmland ; (*b*) consumption per head of farm products and real income per head, and (*c*) the ratio of the prices of farm to non-farm products and the ratio of the consumption of farm products to that calculated from real income per head.

Chapter V contains the main economic argument which, together with certain other material, is summarised in the next section. Chapter VI deals with the supply and demand of capital. Chapter VII discusses historically such topics as the terms of trade, gold production and interest rates. The final chapter is concerned with the problems of 1941–60 and ends with an encomium on the *Carta Mercatoria of Edward I.* For reasons which are nowhere apparent Mr. Clark concludes that " while in the trade of goods at wholesale there should be no differentiation against any merchant or producer on the grounds of his nationality . . . each authority has the right to control those goods and services which are produced and consumed within its own borders ".

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[1] Defined equal to the purchasing power of $1 in the U.S.A. over the average of the period 1925–34.

## III

## THE ECONOMIC ARGUMENT SET OUT

An attempt is here made to set out Mr. Clark's main economic argument. As far as possible the symbolism of the book has been followed.

(i) The original force making for change is the increasing production per occupied person in secondary and tertiary industries. These values, which are written $Q_2$ and $Q_3$, are obtained by extrapolation and are shown in the third and fourth columns of figures on p. 60.

(ii) These values determine the level of agricultural wages $L$. The ratio of these wages to the average of non-agricultural incomes is taken according to circumstances at between one-third and one-half. We may thus write

$$L = l\,Q_{2+3} \qquad \tfrac{1}{3} < l < \tfrac{1}{2} \qquad (1)$$

$l$ is a known co-efficient and $Q_{2+3}$ is the average of non-agricultural incomes. The value of $L$ for different countries is given in the fifth column of figures on p. 60. It will be observed that $l$ does not always lie between the limits stated but the method of calculation and the reason for this are not given.

(iii) In the long run, $L$ determines the money marginal product of agricultural labour, $m'$, by determining the size of the industry. Thus

$$m' = L \qquad (2)$$

(iv) The level of agricultural prices, the prices of secondary and tertiary products being assumed constant and equal to unity, determines the real marginal product of agricultural labour. Writing

$$P = p_1/p_{2+3} \qquad p_{2+3} = 1$$

then the real marginal product of agricultural labour, $m$, is given by

$$m = m'/P \qquad (3)$$

(v) It may be taken, on empirical evidence, that the real average product of agricultural labour, $q_1$, is equal to twice the marginal product. Thus

$$q_1 = \frac{2L}{P} \qquad (4)$$

(vi) Write $Q_0$ for the present average product of agricultural labour raised by 32·5 per cent. to allow for its natural upward trend, and write $W_0$ for the present number engaged in agriculture. It may be taken that for a given country and time, productivity varies inversely with the square root of the numbers employed per unit of land in cultivation. Thus the numbers engaged in agriculture in 1960, $w_1$, is given by

$$w_1 = W_0\left(\frac{Q_0}{q_1}\right)^2 \qquad (5)$$

(vii) Provided that $P$ is known, it is possible to calculate $q_1$ and $w_1$. A number of trial values of $P$ are taken and a value of 1·9 is finally accepted, since this value will balance the world supply and demand of primary products.

(viii) It is now necessary to determine the numbers engaged in secondary and tertiary industry, $w_2$ and $w_3$, aggregate real income, $r$, and the consumption of primary products $f$. $W$ is written for the whole occupied population. The values of $w_2$, $w_3$, $r$ and $f$ depend on the solution of the following equations:

$$\frac{w_3Q_3}{W} = 0{\cdot}695\frac{r}{W} - 0{\cdot}102 \tag{6}$$

This expresses tertiary consumption per head of the occupied population in terms of real income per head. The co-efficients were obtained empirically.

$$W = w_1 + w_2 + w_3 \tag{7}$$

This equation defines the whole occupied population as the sum of the numbers occupied in each of the three branches of activity.

$$\frac{f}{A} = d\left(-0{\cdot}0781\frac{r^2}{A^2} + 0{\cdot}1971\frac{r}{A} + 0{\cdot}0069\right) \tag{8}$$

This expresses food consumption per head of the total population, $A$, in terms of the real income per head of the total population and is reached empirically. The co-efficient $d$ is required when $P \neq 1$ in order to take account of the elasticity of demand for food in response to changes in its wholesale price.

The fourth equation expresses $r$ in terms of the other variables, thus

$$r = w_3Q_3 + w_2Q_2 + f + (w_1q_1 - f)P \tag{9}$$

The last term here expresses the fact that the change in the terms of trade will benefit exporters of primary products at the expense of the exporters of other goods.

The solution of equations (6) to (9) for $w_2$, $w_3$, $f$ and $r$ gives a quadratic in $r$. Inserting different values of $P$ enables a value of $\Sigma f$ to be found equal to $\Sigma w_1q_1$ for the same value of $P$. The summation is taken over all countries.

(ix) The analysis given so far is not followed for all countries. Thus the method of sections (v) to (vii) sometimes gives a change in the agricultural population which is too big or too small. Accordingly, restrictions are imposed on these movements.

First, the agricultural population is not allowed to fall by more than 35 per cent. between now and 1960. Thus, for $P = 1{\cdot}9$ the agricultural populations of Eire, Spain, Portugal, Poland, U.S.S.R., Japan and China, Korea, Formosa, etc., are taken at 65 per cent. of their present value.

Second, in some countries the marginal productivity equilibrium would force the number of agricultural workers to such a point as to leave an insufficient number for secondary and tertiary production. These countries may be divided into two groups (*a*) and (*b*).

In group (*a*), which includes Australia, New Zealand and South America other than Argentina and Uruguay, there is sufficient land to maintain productivity per head as numbers rise. In these cases equation (5) is struck out while to equations (6) to (9) is added a fifth, namely

$$w_2Q_2 = 0\cdot175\,r \tag{10}$$

which provides that at least 17·5 per cent. of real income should be obtained from secondary production.

In group (*b*), which includes Denmark, Argentine and Uruguay, Hungary and, at certain price levels, the Balkans, the best land is fairly fully settled. In these cases equation (4) is struck out while equations (5) and (10) are included with the four equations (6) to (9). The resulting quartic in $w_1$ is solved graphically.

Third, in the case of China, India, South-Eastern Asia and Java where $w_1$ and $q_1$ are known, a restriction is made that at least 15 per cent. of the occupied population must be engaged in tertiary industry. Thus we have

$$w_3 = 0\cdot15\,W \tag{11}$$

from which $w_2$ can be obtained.

In the case of Asia, excluding Japan, and Africa, a term

$$\frac{w_1}{W}f \tag{12}$$

is added to the right-hand side of (9) to allow for the element of imputed income in the transport and distribution of foodstuffs consumed by peasants' families. $w_2$ is deduced and a quadratic in $r$ is then obtained.

In the case of Africa, Western Asia and the Islands: (i) equation (11) is taken to hold; (ii) equation (4) is replaced by

$$q_1 = Q_0 \tag{13}$$

and (iii) equation (5) is struck out. The resulting equation is solved by trial and error.

(x) Since $r/W$ is known, we may calculate capital per occupied person, $x$, from the empirical equation

$$\log\left(\frac{r}{W}\right) = 2\cdot884 - 1\cdot108\log x + 0\cdot323\,(\log x)^2 \tag{14}$$

(xi) At the end of the book, opposite p. 118, there is a graph showing savings per occupied person working 2,500 hours, $s$, say, at given levels of real income per occupied person working 2,500 hours. No equation is given but from the curve on the diagram it would appear to be of the form

$$s = a + b\frac{r}{W} + c\frac{r^2}{W^2} \tag{15}$$

(xii) Knowing the occupied population in 1960, it is possible from (14) to obtain the total amount of capital which will need to exist in each country. From similar information, together with (15) it is possible to calculate the level of savings in 1935-8, 1945 and 1960. By interpolating these figures linearly, summing over the period and deducting sums required for making good war damage, an aggregate figure of savings can be reached. This can be compared with the increase in capital requirements. This comparison is used to tell us (*a*) whether in the aggregate savings over the period are sufficient or insufficient and (*b*) which country will have an export surplus or deficit of savings.

## IV

## The Economic Argument Examined

In setting out the argument it has been assumed that the hypotheses are reasonable and that the facts are well grounded. Each of these questions will now be examined. In this section the facts will still be taken for granted and attention will be directed to the hypotheses.

The general method can be seen from the above account. A condition is imposed on the system that in 1960 primary production in the world as a whole will be in equilibrium : that is to say, at the ruling rate of exchange between primary and other products, primary producers will wish to produce the amount that consumers the world over will wish to purchase. It is this basic assumption that makes it possible to reach a value for $P$. It is not simply that $\Sigma w_1 q_1$ is assumed equal to $\Sigma f$ (the summation being over all countries) since apart from variations in stocks this equality is an identity which would hold irrespective of the value of $P$.

The system of equations in the preceding section must yield a supply and demand curve for primary products as a whole in terms of $P$. From equations (4) and (5) it can be seen that the supply curve in each country is of the form

$$w_1 q_1 = kP$$

where $k = \dfrac{W_0 Q_0^2}{2L}$. This relation is dependent on the assumptions made in equations (1) to (5) and not on any direct attempt to determine the form of the supply curve.

The value of $w_1$ and $q_1$ having been determined for any given value of $P$, the demand is determined from equations (6) to (9) or their variations. But these equations do not express $f$ in terms of $P$, other things being held constant. Real income and the numbers occupied in secondary and tertiary activity, $r$, $w_2$ and $w_3$, all take different values for different values of $P$. In other words, $r$, $w_2$ and $w_3$ in each country take values that depend *inter alia* on the assumption of equilibrium in world primary activity. Further, $P$ has the same value in each country as though each country produced a homogeneous bundle of primary products which it exchanged at home or abroad for a homogeneous bundle of secondary and tertiary goods. The extreme artificiality of this analysis should by now be apparent.

There are further implications of this arrangement of the variables. The quantity of capital does not enter explicitly although a certain variation is implicit in the values given to $Q_0$ and $Q_{2+3}$. Indeed, the chain of reasoning goes the other way, the quantity of capital being determined from income in (14).

No allowance is made for changes in unemployment, which is assumed not to exist, the national income being defined equal to the sum of the products of the numbers occupied and the output per head in each branch of activity.

On the supply side, equations (1) and (4) give

$$\frac{p_1 q_1}{p_{2+3} Q_{2+3}} = 2l$$

that is to say, the ratio of money value produced per head in primary to that in secondary and tertiary industries is a constant in each country. A change in the terms of trade in favour of primary activity is taken therefore to result in a fall in the quantitative output per head rather than an increase in the value of primary output per head in relation to that in other branches of activity. This assumption is the opposite of what is reasonable since one would expect the main effect to be a change in the relative value of production per head in the two groups. The fall in output per head would only come about, assuming diminishing returns, if a change in the terms of trade so stimulated primary output that productivity fell so as to counteract exactly the rise in the price of primary products.

The level of employment in primary industries is found for each country from (5). Thus, if the terms of trade turn in favour of primary products, we assume *via* equations (1) and (4) that this will result in a counterbalancing fall in real output per head. We then find from (5) that the change in employment is equal to the square of the reciprocal of the change in productivity. Employment will therefore rise. The combination of this rise in employment and the fall in productivity will produce a new level of output. The new output will be equal to the old output multiplied by $Q_0/q_1$.

In my opinion this is not a very plausible set of relationships. The constancy of $l$ in equation (1) puts a very severe restriction on the system. Any simple relation between wages in primary activity and net income generated in other industries seems rather improbable.

Second, there is room for doubt whether there is much to justify (2). Certainly there is not on Mr. Clark's figures. The penultimate column of figures on page 38 shows, for a number of countries and periods, the ratio of agricultural wages to agricultural output per head. On the basis of statistics which will be examined later, we are told that the marginal product of agricultural labour is one half the average product. The figures given here range from 0·33 to 0·96. A number of entries for different countries and dates are starred because they relate to cases in which the agricultural population was comparatively stable. According to Mr. Clark, the ratios should be about ½ and he draws attention to the fact that they average 0·51. But the individual entries range from 0·33 to 0·72 and have a standard deviation of about 0·12. It is not sufficient that the average of the entries should be about ½; it is necessary also that their dispersion should be small, a condition which is obviously not fulfilled.

On the demand side, equations (6) to (9), four equations have been selected out of a large number of possible equations but no justification is given of the particular selection made. The main feature of this part of the analysis is the number of variants of the four main equations which are employed in cases, in fact nearly all the cases, where the original four lead to unplausible results.

Mr. Clark has, of course, been careful to keep his equations equal in number to his unknowns: and accordingly he obtains values for his unknowns. It may be said in passing that it is not obvious that in all cases these values are unique. In the case, for example, of the quadratic in $r$ (see p. 56) there will necessarily be two roots and therefore two solutions in $r$. It is to be assumed that Mr. Clark has satisfied himself that only one has a reasonable positive value but there is no discussion of this point. There is nothing, therefore, in the fact that answers come out of the analysis to suggest that they are plausible answers; this must depend on the reasonable nature of the hypotheses and the adequacy of the factual foundation. This remark is trite and obvious but it is necessary as a counterblast to the uncritical dogmatism with which Mr. Clark presents his results.

## V

## The Statistical Basis Examined

So far, we have considered only the economic analysis; we come now to the basis of fact. The more important inter-relations can conveniently be taken in the order in which they appeared in the summary of the economic argument.

First, then, the basis of equation (5). This relationship was obtained by relating output per head, $q$, to employment per unit of area, $w$, by an equation of the form

$$q = aw^{\alpha}$$

$q$ and $w$ were estimated for a number of countries, and $a$ and $\alpha$ were found by fitting a straight line presumably with $q$ as the dependent variable and presumably giving each observation equal weight. The data lie around a line of negative slope but the correlation is far from perfect even allowing for the compression involved in taking logarithms. For example, to take extreme cases, the observed value of $q$ for New

Zealand is well over double the calculated value while in the U.S.S.R. it is about one-fifth.

These are rather considerable discrepancies. They are not, however, surprising. For it is surely not reasonable to expect a simple relation among the data. The type of primary activity is very different in the different countries. Moreover, in extrapolating to 1960, an allowance is made for an upward trend in productivity, that is, the value of *a* is assumed to rise through time. But if this is permitted, and a trend in productivity is clearly reasonable, then there is no guarantee that all the countries are at the same stage of development.

At this point it is convenient to examine the basis for the assumed increase in the productivity of primary workers between 1934–5 and 1960, given a constant number of workers per unit of area. The figure obtained, 32·5 per cent., is found by taking the mean of the figures for Australia, U.S.A., Sweden and Germany over the twenty-five years from 1910. The percentage changes are respectively 23, 39, 49 and 19. In these cases, we are told, workers per unit of area remained approximately constant but it seems an inadequate basis on which to estimate a given percentage increase in all countries.

The basis for equation (6) is a set of estimates for different countries and dates of tertiary output divided by total man hours worked in each economy and total real income divided by the same factor. In some cases the observation is a single year, in others a decade. Again, the assumption is made that the data form a homogeneous group and the use of the relationship implies that if real income per occupied person is given, tertiary "consumption" is known. This means (*a*) that no trend is allowed for between now and 1960; (*b*) that no extraneous factors such as the terms of trade so prominent in other parts of the analysis do or will influence the relationship; and (*c*) that in 1960 all the estimates will be on the line. The latter is, of course, an inevitable assumption since a fitted line is taken to give the most probable values of the dependent variable for given values of the independent variables. In the present instance, however, if there were no change in real income this would mean that in New Zealand the estimate of tertiary consumption would more than double; in Norway and Hungary it would about double; in Britain it would fall by about 15 per cent. These discrepancies exist in the data before any extrapolation takes place.

The constants in equation (8), with the exception of the co-efficient *d*, are obtained in a manner which contains in an exaggerated form all the objections raised against those in equation (6) and several others besides. I shall not discuss them.

The co-efficient *d*, which is intended to take into account the elasticity of demand for food in response to changes in its wholesale price, is based on the following remarkable analysis. The logarithms of the ratio of farm product to non-farm product prices are related to the logarithms of farm products consumed per head as a proportion of consumption expected at the relevant levels of real income per head. These series are given for the United States over the period 1925–38 and for Great Britain over the period 1929–39. In the latter case, *food* prices and consumption only are used. These data are in arbitrary units and are shown on a diagram in arbitrary relative position to each other. It is then proposed to fit a straight line whose slope is the required elasticity of demand. How this is to be done to both sets together is not discussed. However, in fact it is not done and a line is drawn on the diagram which evidently and admittedly does not fit the data. This does not deter Mr. Clark. " It is true," he says, " that the American data for 1925–29 are left well to the right of the line, but it appears that this can be explained in terms of changing habits of consumption. Any alternative hypothesis about the slope of the line would create difficulties much less readily explainable ".

This procedure is lacking in candour. It amounts to setting up an elaborate if inadequate analysis only to knock it down because, one must suppose, it conflicts with other evidence which is not discussed. Finally, the data from which the line is derived show movements of relative prices between 0·8 and 1·1. $P$ in 1960, it will be remembered, is 1·9—a considerable extrapolation. And all this is not a small matter for in the analysis $d$ is about $\frac{2}{3}$. In other words, on the strength of the foregoing primary consumption is reduced by about $\frac{1}{8}$ in each country.

Objections of this order, not mere carping criticism, could be raised against almost all of Mr. Clark's " facts ". I shall do no more than mention such extravagances as the table on p. 17 which shows the anticipated net emigration to all the main countries between 1943 and 1960 and the one on p. 67 which shows the anticipated price in 1960 of each of the main farm products. I shall give only one further example. The diagram opposite p. 118 shows the relation between savings and real income. Anyone possessing the book and with a few moments to spare will find it instructive to join the consecutive time points for those countries for which this can be done.

# Scientism and the Study of Society

## PART II[1]

## V, VI, OBJECTIVISM; VII, VIII, COLLECTIVISM; IX, X, HISTORISM

By F. A. v. HAYEK

### V

The great differences between the characteristic methods of the physical sciences and those of the social sciences make it not difficult to understand why the natural scientist who turns to the works of the professional students of social phenomena should so often feel that he has got among a company of people who habitually commit all the mortal sins which he is most careful to avoid, and that a science of society conforming to his standards does not yet exist. From this to the attempt to create a new science of society which satisfies his conception of Science is but a step. During the last four generations attempts of this kind have been constantly made; and though they have never produced the results which had been expected, and though they did not even succeed in creating that continuous tradition which is the symptom of a healthy discipline, they are repeated almost every month by someone who hopes thereby to revolutionize social thought. Yet, though these efforts are mostly disconnected, they regularly show certain characteristic features which we must now consider. These methodological features can be conveniently treated under the headings of "objectivism", "collectivism", and "historism", corresponding to the "subjectivism", the "individualism", and the theoretical character of the developed disciplines of social study.

The attitude which, for want of a better term, we shall call the "objectivism" of the scientistic approach to the study of man and society, has found its most characteristic expression in the various attempts to dispense with our subjective knowledge of the working of the human mind, attempts which in various forms have affected almost all branches of social study. From Auguste Comte's denial of the possibility of introspection, through various attempts to create an "objective psychology", down to the behaviourism of J. B. Watson and the "physicalism" of O. Neurath, a long series of authors have attempted to do without the knowledge derived from "introspection". But, as can be easily shown, these attempts to avoid the use of knowledge which we possess are bound to break down.

[1] The first part appeared in ECONOMICA for August, 1942.

A behaviourist or physicalist, to be consistent, ought not to begin by observing the reactions of people to what our senses tell us are similar objects; he ought to confine himself to studying the reactions to stimuli which are identical in a strictly physical sense. He ought, e.g., not to study the reactions of persons who are shown a red circle or made to hear a certain tune, but solely the effects of a light wave of a certain length on a particular point of the retina of the human eye, etc., etc. No behaviourist, however, seriously contemplates doing so. They all take it naïvely for granted that what appears alike to us will also appear alike to other people. Though they have no business to do so, they make constant use of the classification of external stimuli by our senses and our mind as alike or unlike, a classification which we know only from our personal experience of it and which is not based on any objective tests showing that these facts also behave similarly to each other. This applies as much to what we commonly regard as simple sense qualities, such as colour, the pitch of sound, smell, etc., as to our perception of configurations (*Gestalten*) by which we classify physically very different things as specimens of a particular "shape", e.g., as a circle or a certain tune. To the behaviourist or physicalist the fact that we recognise these things as similar is no problem.

This naïve attitude, however, is in no way justified by what the development of physical science itself teaches us. As we have seen before,[1] one of the main results of this development is that things that to us appear alike may not be alike in any objective sense, i.e., may have no other properties in common. Once we have to recognise, however, that things differ in their effects on our senses not necessarily in the same way in which they differ in their behaviour towards each other, we are no longer entitled to take it for granted that what to us appears alike or different will also appear so to others. That this is so as a rule is an important empirical fact which, on the one hand, demands explanation (a task for psychology) and which, on the other hand, must be accepted as a basic datum in our study of people's conduct. That different objects mean the same thing to different people, or that different people mean the same thing by different acts, remain important facts though physical science may show that these objects or acts possess no other common properties.

It is true, of course, that we know nothing about other people's minds except through sense perceptions, i.e., the observation of physical facts. But this does not mean that we know nothing but physical facts. Of what kind the facts are with which we have to deal in any discipline is not determined by all the properties possessed by the concrete objects to which the discipline applies, but only by those properties by which we classify them for the purposes of the discipline in question. To take an example from the physical sciences: all levers or pendulums of which we can conceive have chemical and optical properties; but when we talk about levers or pendulums we do not talk about chemical or optical facts. What makes a number of individual phenomena facts of one kind are the attributes which we select in order to treat them as members of one class. And though all social phenomena with which we can possibly be concerned will possess physical attributes, this does not mean that they must be physical facts for our purpose.

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[1] Cf. the first part of this article, pp. 271–274.

The significant point about the objects of human activity with which we are concerned in the social sciences, and about these human activities themselves, is that in interpreting human activities we spontaneously and unconsciously class together as instances of the same object or the same act any one of a large number of physical facts which may have no physical property in common. We know that other people like ourselves regard any one of a large number of physically different things, *a*, *b*, *c*, *d*, . . . etc., as belonging to the same class; and we know this because other people, like ourselves, react to any one of these things by any one of the movements $\alpha$, $\beta$, $\gamma$, $\delta$, . . . which again may have no physical property in common. Yet this knowledge on which we constantly act, which must necessarily precede, and is pre-supposed by, any communication with other men, is not conscious knowledge in the sense that we are in a position exhaustively to enumerate all the different physical phenomena which we unhesitatingly recognise as a member of the class: we do not know which of many possible combinations of physical properties we shall recognise as a certain word, or as a "friendly face" or a "threatening gesture". Probably in no single instance has experimental research yet succeeded in precisely determining the range of different phenomena which we unhesitatingly treat as meaning the same thing to us as well as to other people; yet we constantly and successfully act on the assumption that we do classify these things in the same manner as other people do. We are not in a position—and may never be in the position—to substitute objects defined in physical terms for the mental categories we employ in talking about other people's actions.[1] Whenever we do so the physical facts to which we refer are significant not as physical facts, i.e., not as members of a class all of which have certain physical properties in common, but as members of a class of what may be physically completely different things which "mean" the same thing to us.

It becomes necessary here to state explicitly a consideration which is implied in the whole of our argument on this point and which, though it seems to follow from the modern conception of the character of physical research, is yet still somewhat unfamiliar. It is that not only those mental entities, such as "concepts" or "ideas", which are commonly

[1] The attempts often made to evade this difficulty by an *illustrative* enumeration of some of the physical attributes by which we recognise the object as belonging to one of these mental categories are just begging the question. To say that when we speak about a man being angry we mean that he shows certain physical symptoms helps us very little unless we can exhaustively enumerate all the symptoms by which we ever recognise, and which always when they are present mean, that the man who shows them is angry. Only if we could do this would it be legitimate to say that in using this term we mean no more than *certain* physical phenomena.

recognised as " abstractions ", but *all* mental phenomena, sense perceptions and images as well as the more abstract " concepts " and " ideas " must be regarded as acts of classification performed by the brain.[1] This is, of course, merely another way of saying that the qualities which we perceive are not properties of the objects but ways in which we (individually or as a race) have learnt to group or classify external stimuli. To perceive is to assign to a familiar category (or categories) : we could not perceive anything completely different from everything else we have ever perceived before.[2] This does not mean, however, that everything which we actually class together must possess common properties other than that we react in the same way to these things. It is a common but dangerous error to believe that things which our senses or our mind treat as members of the same class must have something else in common beyond being registered in the same manner by our mind. Although there will usually exist some objective justification of why we regard certain things as similar in kind, this need not always be the case. But while in our study of nature classifications which are not based on any similarity in the behaviour of the objects towards each other must be treated as "deceptions" of which we must free ourselves, they are of positive significance in our attempts to understand human action. The important difference between the position of these mental categories in the two spheres is that when we study the working of external nature our sensations and thoughts are not links in the chain of observed events—they are merely about them ; but in the mechanism of society they form an essential link, the forces here at work operate through these mental entities which are directly known to us : while the things in the external world do not behave alike or differently because they appear alike to us, we do behave in a similar or different manner because the things appear alike or different to us.

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[1] This must also serve as a justification for what may have seemed the very loose way in which we have throughout, in illustrative enumerations of mental entities, indiscriminately lumped together such concepts as " sensation ", " perceptions ", " concepts ", or " ideas ". These different types of mental entities all have in common that they are classifications of possible external stimuli (or complexes of such stimuli). This contention will perhaps appear less strange now than would have been the case fifty years ago, since in the configurations or *Gestalt* qualities we have become familiar with something which is intermediate between the old " elementary " sense qualities and concepts. It may be added that on this view there would, however, seem to be no justification for the unwarranted ontological conclusions which many members of the *Gestalt* school like to draw from their interesting observations ; there is no reason to assume that the " wholes " which we perceive are properties of the external world and not merely ways in which our mind classifies complexes of stimuli ; like other abstractions, the relations between the parts thus singled out may be significant or not.

Perhaps it should also be mentioned here that there is no reason to regard values as the only purely mental categories which do therefore not appear in our picture of the physical world. Although values must necessarily occupy a central place wherever we are concerned with purposive action, they are certainly not the only kind of purely mental categories which we shall have to employ in interpreting human activities : the distinction between true and false provides at least one other instance of such purely mental categories which is of great importance in this connection. On the connected point that it is not necessarily value considerations which will guide us in selecting the aspects of social life which we study, see below p. 55, footnote 2.

[2] Although the second time we are exposed to a new stimulus we may already " recognise " it as identical with what happened to us in circumstances which its recurrence calls to our mind, we should still not have been "conscious" of it on the first occasion when it had not yet acquired a place in the structure of our mind.

The behaviourist or physicalist who wished in studying human behaviour really to avoid using the categories which we find ready in our mind, and who wanted to confine himself strictly to the study of man's reactions to objects defined in physical terms, would consistently have to refuse to say anything about human actions till he had experimentally established how our senses and our mind group external stimuli as alike or unlike. He would have to begin by asking which physical objects appear alike to us and which do not (and how it comes about that they do) before he could seriously undertake to study human behaviour towards these things.

It is important to observe that our contention is not that such an attempt to explain how our mind or our brain transforms physical facts into mental entities is in principle impossible. Once we recognise this as a process of classification there is no reason why we should not learn to understand the principle on which it operates. Classification is, after all, a mechanical process, i.e., a process which could be performed by a machine which " sorts out " and groups objects according to certain properties.[1] Our argument is, rather, in the first instance, that for the task of the social sciences such an explanation of the formation of mental entities and their relations to the physical facts which they represent is unnecessary, and that such an explanation would help us in no way in our task; and, secondly, that such an explanation, although conceivable, is not only not available at present and not likely to be available for a long time yet, but also unlikely to be ever more than an " explanation of the principle "[2] on which this mechanism of classification works. It would seem that any mechanism of classification would always have to possess a degree of complexity greater than any one of the different things which it classifies; and if this is correct it would follow that it is impossible that our brain should ever be able to produce a complete explanation (as distinguished from a mere explanation of the principle) of the particular ways in which it itself classifies external stimuli. We shall later have to consider the significance of the related paradox that to explain all our knowledge would require that we should know more than we actually do, which is, of course, a self-contradictory statement.

But let us assume for the moment that we had actually succeeded in fully reducing all mental phenomena to physical processes. Assume that we knew the mechanism by which our central nervous system groups any one of the (elementary or complex) stimuli $a, b, c, \ldots$ or $, m, n, \ldots$ or $r, s, t, \ldots$ into definite classes determined by the fact that to any member of one class we shall react by any one of the members of the corresponding classes or reactions $\alpha, \beta, \gamma, \ldots$ or $\nu, \xi, o, \ldots$ or $\phi, \chi, \psi \ldots$ This assumption implies both that this system is not merely familiar to us as the way in which our own mind acts, but that we explicitly know all the relations by which it is determined, and that we also know the mechanism by which the classification is actually effected. We should then be able strictly to correlate the mental entities with definite groups of physical facts. We should thus have " unified " science, but we should be in no better position with respect to the specific task of the social sciences than we are now. We should still have to use the old categories, though we should be able to explain their formation and though we should know the physical facts " behind " them. Although we should know that a different arrangement of the facts of nature is more appropriate for explaining external events, in interpreting human actions we should still have to use the classification in which these facts actually appear in the minds of the acting people. Thus, quite apart from the fact that we should probably have to wait forever till we were able to substitute physical facts for the mental entities, even if this were achieved we should be no better equipped for the task we have to solve in the social sciences.

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[1] Which, as we have already seen, does, of course, not mean that it will always treat only elements which have common properties as members of the same class.

[2] Cf. the concluding paragraphs of the first part of this article.

The idea, implied in Comte's hierarchy of the sciences[1] and in many similar arguments, that the social sciences must in some sense be " based " on the physical sciences, that they can only hope for success after the physical sciences have advanced far enough to enable us to treat social phenomena in physical terms, in " physical language ", is, therefore, entirely erroneous. The problem of explaining mental processes by physical ones is entirely distinct from the problems of the social sciences, it is a problem for physiological psychology. But whether it is solved or not, for the social sciences the given mental entities must provide the starting point, whether their formation has been explained or not.

## VI

We cannot here discuss all the other forms in which the characteristic " objectivism " of the scientistic approach has made itself felt and led to error in the social sciences. We shall, in the course of our historical survey, find this tendency to look for the " real " attributes of the objects of human activity which lie behind men's views about them, represented in a great many different ways. Only a brief survey of them can here be attempted.

Nearly as important as the various forms of behaviourism, and closely connected with them, is the common tendency to attempt in the study of social phenomena to disregard all the " merely " qualitative phenomena and to concentrate, on the model of the natural sciences, on the quantitative aspects, on what is measurable. We have seen before[2] how in the natural sciences this tendency is a necessary consequence of their specific task of replacing the picture of the world in terms of sense qualities by one in which the units are defined exclusively by their explicit relations. Its success here has brought it about that it has come to be regarded as the hall-mark of all genuinely scientific procedure. Yet its *raison d'être*, the need to replace the classification of events which our senses and our mind provide by a more appropriate one, is absent where we try to understand human beings, and where this understanding is made possible by the fact that we have a mind like theirs, and that from the mental categories we have in common with them we can reconstruct the social complexes which are our concern. The blind transfer of the striving for quantitative measurements[1] to a field where the specific conditions are not present which give it its basic importance in the natural sciences is the result of an entirely unfounded prejudice. It is probably responsible for the worst aberrations and absurdities produced by scientism in the social sciences. It not only leads frequently to the selection for study of the most irrelevant aspects of the phenomena because they happen to be measurable, but also to " measurements " and assignments of numerical values which are absolutely meaningless. What a distinguished philosopher recently wrote about psychology is at least equally true of the social sciences, namely that it is only too easy " to rush off to measure something without considering what it is we are measuring, or what measurement means. In this respect some recent measurements are of the same logical type as Plato's determination that a just ruler is 729 times as happy as an unjust one."[2]

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[1] Cf. the comment on this by Carl Menger in the passage quoted before, ECONOMICA, August, 1942, p. 287, footnote.

[2] Cf. the first part of this article, ECONOMICA, August, 1942, pp. 272, 275.

Closely connected with the tendency to treat the objects of human activity in terms of their " real " attributes instead of as what they appear to the acting people is the propensity to conceive of the student of society as endowed with a kind of super-mind, some sort of absolute knowledge, which makes it unnecessary for him to start from what is known by the people whose actions he studies. Among the most characteristic manifestations of this tendency are the various forms of social " energetics " which, from the earlier attempts of Ernest Solvay, Wilhelm Ostwald and F. Soddy down to our own day[3] have constantly reappeared among scientists and engineers approaching the problems of social organisation. The idea underlying these theories is that, as science is supposed to teach that everything can be ultimately reduced to quantities of energy, man in his plans should treat the various things not according to the concrete usefulness they possess for the purposes for which he knows how to use them, but as the interchangeable units of abstract energy which they " really " are.

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[1] It should, perhaps, be emphasised that there is no necessary connection between the use of mathematics in the social sciences and the attempts to measure social phenomena—as particularly people who are acquainted only with elementary mathematics are apt to believe. Mathematics may—and in economics probably is—absolutely indispensable to describe certain types of complex structural relationships, though there may be no chance of ever knowing the numerical values of the concrete magnitudes (misleadingly called " constants ") which appear in the formulæ describing these structures.

[2] M. R. Cohen, *Reason and Nature*, p. 305.

[3] Cf. L. Hogben (in *Lancelot Hogben's Dangerous Thoughts*, 1939, p. 99): " Plenty is the excess of free energy over the collective calory debt of human effort applied to securing the needs which all human beings share ".

Another, hardly less crude, and even more widespread, example of this tendency is the conception of the " objective " possibilities of production, of the quantity of social output which the physical facts are supposed to make possible, an idea which frequently finds expression in quantitative estimates of the supposed " productive capacity " of society as a whole. These estimates regularly refer, not to what men can produce by means of any stated organisation, but to what in some undefined " objective " sense " could " be produced from the available resources. Most of these assertions have no ascertainable meaning whatever. They do not mean that *x* or *y* or any particular organisation of people could achieve these things. What they amount to is that *if* all the knowledge dispersed among many people could be mastered by a single mind, and *if* this master-mind could make all the people act at all times as he wished, certain results could be achieved; but these results could, of course, not be known to any except such a master-mind. It need hardly be pointed out that an assertion about a " possibility " which is dependent on such conditions has no relation to reality. There is no such thing as a productive capacity of society in the abstract—apart from particular forms of organisation. The only fact which we can regard as given is that there are particular people who have certain concrete knowledge about the way in which particular things can be used for particular purposes. This knowledge never exists as an integrated whole or in one mind, and the only knowledge that can in any sense be said to exist are these separate and often inconsistent and even conflicting views of different people.

Of a very similar nature are the frequent statements about the " objective " needs of the people, where " objective " is merely a name for somebody's views about what the people ought to want. We shall have to consider further manifestations of this " objectivism " more fully towards the end of this article when we turn from the consideration of scientism proper to the effects of the characteristic outlook of the engineer, whose conceptions of " efficiency " have been one of the most powerful vehicles through which this attitude has affected current views on social problems.

## VII

Closely connected with the " objectivism " of the scientistic approach is its methodological collectivism, its tendency to treat " wholes " like " society " or the " economy ", " capitalism " (as a given historical " phase ") or a particular " industry " or " class " or " country " as definitely given objects about which we can discover laws by observing their behaviour as wholes. While the specific subjectivist approach of the social sciences starts, as we have seen, from our knowledge of the inside of these social complexes, the knowledge of the individual attitudes which form the elements of their structure, the objectivism of the natural sciences tries to view them from the outside[1]; it treats social phenomena not as something of which the human mind is a part and the principles of whose organisation we can reconstruct from the familiar parts, but as if they were objects directly perceived by us as wholes.

There are several reasons why this tendency should so frequently show itself with natural scientists. They are used to seek first for empirical regularities in the relatively complex phenomena that are immediately given to observation, and only after they have found such regularities to try and explain them as the product of a combination of other, often purely hypothetical, elements (constructs) which are assumed to behave according to simpler and more general rules. They are therefore inclined in the social field, too, to seek first for empirical regularities in the behaviour of the complexes before they feel that there is need for a theoretical explanation. This tendency is further strengthened by the experience that there are few regularities in the behaviour of individuals which can be established in a strictly objective manner; and they turn therefore to the wholes in the hope that they will show such regularities. Finally, there is the rather vague idea that as " social phenomena " are to be the object of study, the obvious procedure is to start from the direct observation of these " social phenomena ", where the existence in popular usage of such terms as " society " or " economy " is naïvely taken as evidence that there must be definite " objects " corresponding to them. The fact that people all talk about " the nation " or " capitalism " leads to the belief that the first step in the study of these phenomena must be to go and see what they are like, just as we should if we heard about a particular stone or a particular animal.[2]

The error involved in this collectivist approach is that it mistakes for facts what are no more than provisional theories, models constructed by the popular mind to explain the connection between some of the individual phenomena which we observe. The paradoxical aspect of it, however, is, as we have seen before,[3] that those who by the scientistic prejudice are led to approach social phenomena in this manner are, by their very anxiety to avoid all merely subjective elements and to confine themselves to " objective facts ", induced to commit the mistake they are most anxious to avoid, namely to treat as facts what are no more than vague popular theories. They thus become, when they least suspect it, the victims of the fallacy described by an expressive German term as *Begriffsrealismus* and made familiar to us by Professor Whitehead as the " fallacy of misplaced concreteness ".

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[1] The description of this contrast as one between the view from the inside and the view from the outside, though, of course, metaphorical, is less misleading than such metaphors usually are and perhaps the best short way to indicate the nature of the contrast. It brings out that what of social complexes is directly known to us are only the parts and that the whole is never directly perceived but always reconstructed by an effort of our imagination.

[2] It would, of course, be false to believe that the first instinct of the student of social phenomena is any less to " go and see". It is not ignorance of the obvious but long experience which has taught him that directly to look for the wholes which popular language suggests to exist leads nowhere. It has, indeed, rightly become one of the first maxims which the student of social phenomena learns (or ought to learn) never to speak of " society " or a " country " acting or behaving in a certain manner, but always and exclusively to think of individuals as acting.

[3] Cf. Part I of this article, p. 286.

The naïve realism which uncritically assumes that where there are commonly used concepts there must also be definite " given " things which they describe is so deeply embedded in current thought about social phenomena that to free ourselves from it needs a deliberate effort of will. While most people will readily admit that in this field there may exist special difficulties in recognising definite wholes because we have never before us many specimens of a kind and therefore cannot readily distinguish their constant from their merely accidental attributes, few are aware that there is a much more fundamental obstacle : that the wholes as such are never given to our observation but are without exception constructions of our mind. They are not " given facts ", objective data of a similar kind which we spontaneously recognise as similar by their common physical attributes. They cannot be perceived at all apart from a mental scheme that shows the connection between some of the many individual facts which we can observe. Where we have to deal with such social wholes we cannot, as we do in the natural sciences, start from the observation of a number of instances which we recognise spontaneously by their common sense attributes as instances of " societies " or " economies ", " capitalisms " or " nations ", " languages " or " legal systems ", and where only after we have collected a sufficient number of instances we begin to seek for common laws which they may obey. Social wholes are not given to us as what we may call " natural units " which we recognise as similar with our senses, as we do with flowers or butterflies, minerals or light-rays, or even forests or ant-heaps. They are not given to us as similar things before we even begin to ask whether what looks alike to us also behaves in like manner. The terms for collectives which we all readily use do not designate definite things in the sense of stable collections of sense attributes which we recognise as alike by inspection ; they refer rather to certain structures of relationships between some of the many things we can observe within given spatial and temporal limits and which we select because we think we can discern connections between them—connections which may or may not exist in fact.

What we group together as instances of the same collective or whole are different complexes of individual events, in themselves perhaps quite dissimilar, but believed by us to be related to each other in a similar manner ; they are classifications or selections of certain elements of a complex picture on the basis of a theory about their coherence. They do not stand for definite things or classes of things (if we understand the term " thing " in any material or concrete sense) but for a pattern or order in which different things may be related to each other—an order which is not a spatial or temporal order but can be defined only in terms of relations which are intelligible human attitudes. This order or pattern is as little perceptible as a physical fact as these relations themselves; and it can be studied only by following up the implications of the particular combination of relationships. In other words, the wholes about which we speak exist only if, and to the extent to which, the theory is correct which we have formed about the connection of the parts which they imply and which we can explicitly state only in the form of a model built from those relationships.[1]

The social sciences, thus, do not deal with "given" wholes but their task is to *constitute* these wholes by constructing models from the familiar elements—models which reproduce the structure of relationships between some of the many phenomena which we always simultaneously observe in real life. This is no less true of the popular concepts of social wholes which are represented by the terms current in ordinary language; they too refer to mental models, but instead of a precise description they convey merely vague and indistinct suggestions of the way in which certain phenomena are connected. Sometimes the wholes constituted by the theoretical social sciences will roughly correspond with the wholes to which the popular concepts refer, because popular usage has succeeded in approximately separating the significant from the accidental; sometimes the wholes constituted by theory may refer to entirely new structural connections of which we did not know before systematic study commenced and for which ordinary language has not even a name. If we take current concepts like those of a "market" or of "capital", the popular meaning of these words corresponds at least in some measure to the similar concepts which we have to form for theoretical purposes, although even in these instances the popular meaning is far to vague to allow the use of these terms without first giving them a more precise meaning. If they can be retained in theoretical work at all it is, however, because in these instances even the popular concepts have long ceased to describe particular concrete things, definable in physical terms, and have come to cover a great variety of different things which are classed together solely because of a recognised similarity in the structure of the relationships between men and things. A "market", e.g., has long ceased to mean only the periodical meeting of men at a fixed place to which they bring their products to sell them from temporary wooden stalls. It now covers any arrangements for regular contacts between potential buyers and sellers of any thing that can be sold, whether by personal contact, by telephone or telegraph, by advertising, etc., etc.[2]

When, however, we speak of the behaviour of, e.g., the "price system" as a whole and discuss the complex of connected changes which will correspond in certain conditions to a fall in the rate of interest, we are not concerned with a whole that obtrudes itself on popular notice or that is ever definitely given; we can only reconstruct it by following up the reactions of many individuals to the initial change and its immediate effects. That in this case certain changes " belong together ", that among the large number of other changes which in any concrete situation will always occur simultaneously with them and will often swamp those which form part of the complex in which we are interested, a few form a more closely interrelated complex, we do not know from observing that these particular changes regularly occur together. That would indeed be impossible because what in different circumstances would have to be regarded as the same set of changes could not be determined by any of the physical attributes of the things but only by singling out certain relevant aspects in the attitudes of men towards the things; and this can be done only by the help of the models we have formed.

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[1] Cf. F. Kaufmann, "Soziale Kollektiva," *Zeitschrift für Nationalökonomie*, Vol. I, 1930.

[2] It should be noted that though observation may assist us to understand what people mean by the terms they use, it can never tell us what a "market" or "capital", etc., really are, i.e., which are the significant relations which it is useful to single out and combine into a model.

The mistake of treating as definite objects " wholes " that are no more than constructions, and that can have no properties except those which follow from the way in which we have constructed them from the elements, has probably appeared most frequently in the form of the various theories about a " social " or " collective " mind[1] and has in this connection raised all sorts of pseudo-problems. The same idea is frequently but imperfectly concealed under the attributes " personality " or " individuality " which are ascribed to society. Whatever the name, these terms always mean that, instead of reconstructing the wholes from the relations between individual minds which we directly know, a vaguely apprehended whole is treated as something akin to the individual mind. It is in this form that in the social sciences an illegitimate use of anthropomorphic concepts has had as noxious an effect as the use of such concepts in the natural sciences. The remarkable thing here is, again, that it should so frequently be the empiricism of the positivists, the arch-enemies of any anthropomorphic concepts even where they are in place, which leads them to postulate such metaphysical entities and to treat humanity, as for instance Comte does, as one " social being," or as a kind of super-person. But as there is no other possibility than either to compose the whole from the individual minds or to postulate a super-mind in the image of the individual mind, and as positivists reject the first of these alternatives, they are necessarily driven to the second. We have here the root of that curious alliance between 19th century positivism and Hegelianism which will occupy us in a later study.

The collectivist approach to social phenomena has not often been so emphatically proclaimed as when the founder of sociology, Auguste Comte, asserted with respect to them that, as in biology, " the whole of the object is here certainly much better known and more immediately accessible "[1] than the constituent parts. This view has exercised a lasting influence on that scientistic study of society which he attempted to create. Yet the particular similarity between the objects of biology and those of sociology, which fitted so well in Comte's hierarchy of the sciences, does not in fact exist. In biology we do indeed first recognise as things of one kind natural units, stable combinations of sense properties, of which we find many instances which we spontaneously recognise as alike. We can, therefore, begin by asking why these definite sets of attributes regularly occur together. But where we have to deal with social wholes or structures it is not the observation of the regular coexistence of certain physical facts which teaches us that they belong together or form a whole. We do not first observe that the parts always occur together and afterwards ask what holds them together; but it is only because we know the ties that hold them together that we can select a few elements from the immensely complicated world around us as parts of a connected whole.

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[1] On this whole problem, see M. Ginsberg, *The Psychology of Society*, 1921, chapter IV.—What is said in the text does of course not preclude the possibility that our study of the way in which individual minds interact may reveal to us a structure which operates in some respects similarly to the individual mind. And it might be possible that the term collective mind would prove the best term available to describe such structures—though it is most unlikely that the advantages of the use of this term would ever outweigh its disadvantages. But even if this were the case the employment of this term should not mislead us into thinking that it describes any observable object that can be directly studied.

We shall presently see that Comte and many others regard social phenomena as given wholes in yet another, different, sense, contending that concrete social phenomena can be understood *only* by considering the totality of everything that can be found within certain spatio-temporal boundaries, and that any attempt to select parts or aspects as systematically connected is bound to fail. In this form the argument amounts to a denial of the possibility of a theory of social phenomena as developed, e.g., by economics, and leads directly to what has been misnamed the " historical method " with which, indeed, methodological collectivism is closely connected. We shall have to discuss this view below under the heading of " historism ".

## VIII

The endeavour to grasp social phenomena as " wholes " finds its most characteristic expression in the tendency to gain a distant and comprehensive view in the hope that thus regularities will reveal themselves which remain obscure at closer range. Whether it is the conception of an observer from a distant planet, which has always been a favourite with positivists from Condorcet to Mach,[2] or whether it is the survey of long stretches of time through which it is hoped that constant configurations or regularities will reveal themselves, it is always the same endeavour to get away from our inside knowledge of human affairs and to gain a view as, it is supposed, would be commanded by somebody who was not himself a man but stood to men in the same relation as that in which we stand to the external world.

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[1] *Cours de philosophie positive*, Vol. IV (2nd–4th ed.), p. 258.

[2] Cf. Ernst Mach, *Erkenntnis und Irrtum*, 3rd ed., 1917, p. 28, where, however, he points out correctly that " Könnten wir die Menschen aus grösserer Entfernung, aus der Vogelperspektive, vom Monde aus beobachten, so würden die feineren Einzelheiten mit den von individuellen Erlebnissen herrührenden Einflüssen für uns verschwinden, und wir würden nichts wahrnehmen, als Menschen, die mit grosser Regelmässigkeit wachsen, sich nähren, sich fortpflanzen."

This distant and comprehensive view of human events at which the scientistic approach aims is now often described as the " macroscopic view ". It would probably be better called the telescopic view (meaning simply the distant view—unless it be the view through the inverted telescope !) since its aim is deliberately to ignore what we can see only from the inside. In the " macrocosm " which this approach attempts to see, and in the " macrodynamic " theories which it endeavours to produce, the elements would not be individual human beings but collectives, constant configurations which, it is presumed, could be defined and described in strictly objective terms.

In most instances this belief that the total view will enable us to distinguish wholes by objective criteria proves, however, to be just an illusion. This becomes evident as soon as we seriously try to imagine of what the macrocosm would consist if we were really to dispense with our knowledge of what things *mean* to the acting men, and if we merely observed the actions of men as we observe an ant-heap or a bee-hive. In the picture such a study could produce there could not appear such things as means or tools, commodities or money, crimes or punishments, or words or sentences ; it could contain only physical objects defined in terms of the sense attributes they present to the observer. And since the human behaviour towards the physical objects would show practically no regularities discernable to the observer, since men would in a great many instances not appear to react alike to things which would seem the same to the observer, nor differently to what appeared as different to him, he could not hope to achieve an explanation of their actions unless he had first succeeded in reconstructing in all detail the way in which men's senses and men's minds pictured the external world to them. The famous observer from Mars, in other words, before he could understand even as much of human affairs as the ordinary man does, would have to reconstruct from our behaviour those immediate data of our mind which to us form the starting-point of any interpretation of human action.

If we are not more aware of the difficulties which would be encountered by an observer not possessed of a human mind this is so because we never seriously imagine the possibility that any being with which we are familiar might command sense perceptions or knowledge which are denied to us. Rightly or wrongly we tend to assume that other minds we encounter can differ from ours only by being inferior, so that everything which they perceive or know can also be perceived or be known to us. The only way in which we can form an approximate idea of what would be our position if we had to deal with an organism as complicated as ours but organised on a different principle, so that we should not be able to reproduce its working on the analogy of our own mind, is to conceive that we had to study the behaviour of people with a knowledge vastly superior to our own. If, e.g., we had developed our modern scientific technique while still confined to part of our planet, and then made contact with other parts inhabited by a race which had advanced knowledge much further, we clearly could not hope to understand many of their actions by merely observing what they did and without directly learning from them their knowledge. It would not be from observing them in action that we should acquire their knowledge, but it would be through being taught their knowledge that we should learn to understand their actions.

There is yet another argument which we must briefly consider which supports the tendency to look at social phenomena " from the outside " and which is easily confused with the methodological collectivism of which we have spoken though it is really distinct from it. Are not social phenomena, it may be asked, from their definition mass phenomena, and is it not obvious, therefore, that we can hope to discover regularities in them only if we investigate them by the method developed for the study of mass phenomena, i.e., statistics? Now this is certainly true of the study of certain phenomena, such as those which form the object of vital statistics and which, as has been mentioned before, are sometimes also described as social phenomena, although they are essentially distinct from those with which we are here concerned. Nothing is more instructive than to compare the nature of these statistical wholes, to which the same word "collective" is sometimes also applied, with that of the wholes or collectives with which we have to deal in the theoretical social sciences. The statistical study is concerned with the attributes of individuals, though not with attributes of particular individuals, but with attributes of which we know only that a certain quantitatively determined proportion of all the individuals in our " collective " or " population " possess them. In order that any collection of individuals should form a true statistical collective it is even necessary that the attributes of the individuals whose frequency distribution we study should not be systematically connected or, at least, that in our selection of the individuals which form the " collective " we are not guided by any knowledge of such a connection. The " collectives " of statistics on which we study the regularities produced by the " law of large numbers " are thus emphatically not wholes in the sense in which we describe social structures as wholes. This is best seen from the fact that the properties of the " collectives " which statistics studies must remain unaffected if from the total of elements we select at random a certain part. Far from dealing with structures of relationships, statistics deliberately and systematically disregard the relationships between the individual elements. It is, to repeat, concerned with the properties of the *elements* of the " collective ", though not with the properties of particular elements, but with the frequency with which elements with certain properties occur among the total. And, what is more, it assumes that these properties are *not* systematically connected with the different ways in which the elements are related to each other.

The consequence of this is that in the statistical study of social phenomena the structures with which the theoretical social sciences are concerned actually disappear. Statistics may supply us with very interesting and important information about what is the raw material from which we have to reproduce these structures, but it can tell us nothing about these structures themselves. In some field this is immediately obvious as soon as it is stated. That the statistics of words can tell us nothing about the structure of a language will hardly be denied. But although the contrary is sometimes suggested, the same holds no less true of other systematically connected wholes such as, e.g., the price system. No statistical information about the elements can explain to us the properties of the connected wholes. Statistics could produce knowledge of the properties of the wholes only if it had information about statistical collectives, the elements of which were wholes, i.e., if we had statistical information about the properties of many languages, many price systems, etc. But, quite apart from the practical limitations imposed on us by the limited number of instances known to us, there is an even more serious obstacle to the statistical study of these wholes : the fact which we have already discussed, that these wholes and their properties are not given to our observation but can only be formed or constituted by us from their parts.

What we have said applies, however, by no means to all that goes by the name of statistics in the social sciences. Much that is thus described is not statistics in the strict modern sense of the term, does not deal with mass phenomena at all, but is called statistics only in the older, wider sense of the word in which it is used for any descriptive information about the State or society. Though the term will to-day be used only where this description is of a quantitative nature, this should not lead us to confuse it with the science of statistics in the narrower sense. Most of the economic statistics which we ordinarily meet, such as trade statistics, figures about price changes, and most " time series ", or statistics of the " national income ", are not data to which the technique appropriate to the investigation of mass phenomena can be applied. They are just " measurements " and frequently measurements of the type already discussed at the end of Section VI above. If they refer to significant phenomena they may be very interesting as information about the conditions existing at a particular moment. But unlike statistics proper, which may indeed help us to discover important regularities in the social world (though regularities of an entirely different order from those with which the theoretical sciences of society deal), there is no reason to expect that these measurements will ever reveal anything to us which is of significance beyond the particular place and time at which they have been taken. That they cannot produce generalisations does, of course, not mean that they may not be useful, even very useful; they will often provide us with the data to which our theoretical generalisation must be applied to be of any practical use. They are an instance of the historical information about a particular situation the significance of which we must further consider in the next sections.

## IX

To see the " historism " to which we must now turn described as a product of the scientistic approach may cause surprise since it is usually represented as the opposite to the treatment of social phenomena on the model of the natural sciences. But the view for which this term is properly used (and which must not be confused with the true method of historical study) proves on closer consideration to be a result of the same prejudices as the other typical scientistic misconceptions of social phenomena. If the suggestion that historism is a form rather than the opposite of scientism has still somewhat the appearance of a paradox this is so because the term is used in two different and in some respect opposite and yet frequently confused senses : for the older view which justly contrasted the specific task of the historian to that of the scientist and which denied the possibility of a theoretical science of history, and for the later view which, on the contrary, affirms that history is the only road which can lead to a theoretical science of social phenomena. However great is the contrast between these two views sometimes called " historism " if we take them in their extreme forms, they have yet enough in common to have made possible a gradual and almost unperceived transition from the historical method of the historian to the scientistic historism which attempts to make history a " science " and the only science of social phenomena.

The older historical school, whose growth has recently been so well described by the German historian Meinecke, though under the misleading name of historism,[1] arose mainly in opposition to certain generalising and " pragmatic " tendencies of some, particularly French, 18th century views. Its emphasis was on the singular or unique (*individuell*) character of all historical phenomena which could be understood only genetically as the joint result of many forces working through long stretches of time. Its strong opposition to the " pragmatic " interpretation, which regards social institutions as the product of conscious design, implies in fact the use of a " compositive " theory which explains how such institutions can arise as the unintended result of the separate actions of many individuals. It is significant that among the fathers of this view Edmund Burke is one of the most important and Adam Smith occupies an honourable place.

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[1] G. Meinecke, *Die Entstehung des Historismus*, 1936. The term historism applied to the older historical school discussed by Meinecke is inappropriate and misleading since it was introduced by Carl Menger (*Die Irrthümer des Historismus*, 1884) to describe the distinguishing features of the younger historical school in economics of Schmoller and his associates. Nothing shows more clearly the difference between this younger historical school and the earlier movement from which it inherited the name than that it was Schmoller who accused Menger of being an adherent of the " Burke-Savigny school " and not the other way round. (Cf. G. Schmoller, " Zur Methodologie der Staats- und Sozialwissenschaften," *Jahrbuch für Gesetzgebung*, etc., N.F., Vol. VII, 1886, p. 250).

Yet, although this historical method implies theory, i.e., an understanding of the principles of structural coherence of the social wholes, the historians who employed it not only did not systematically develop such theories and were hardly aware that they used them ; but their just dislike of any generalisation about historical developments also tended to give their teaching an anti-theoretical bias which, although originally aimed only against the wrong kind of theory, yet created the impression that the main difference between the methods appropriate to the study of natural and to that of social phenomena was the same as that between theory and history. This opposition to theory of the largest body of students of social phenomena made it appear as if the difference between the theoretical and the historical treatment was a necessary consequence of the differences between the objects of the natural and the social sciences ; and the belief that the search for general rules must be confined to the study of natural phenomena, while in the study of the social world the historical method must rule, became the foundation on which later historism grew up. But while historism retained the claim for the pre-eminence of historical research in this field, it almost reversed the attitude of the older historical school to history and under the influence of the scientistic currents of the age came to represent history as the empirical study of society from which ultimately generalisation would emerge. History was to be the source from which a new science of society would spring, a science which should at the same time be historical and yet produce what theoretical knowledge we could hope to gain about society.

We are here not concerned with the actual steps in that process of transition from the older historical school to the historism of the younger. It may just be noticed that historism in the sense in which the term is here used, was created not by historians but by students of the specialised social sciences, particularly economists, who hoped thereby to gain an empirical road to the theory of their subject. But to trace this development in detail and to show how the men responsible for it were actually guided by the scientistic views of their generation must be left to the later historical account.[1]

The first point we must briefly consider is the nature of the distinction between the historical and the theoretical treatment of any subject which makes it in fact a contradiction in terms to demand that history should become a theoretical science or that theory should ever be " historical ". If we understand that distinction, it will become clear that it has no necessary connection with the difference of the concrete objects with which the two methods of approach deal and that for the understanding of any concrete phenomenon, be it in nature or in society, both kinds of knowledge are equally required.

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[1] Although in its German origins the connection of historism with positivism is less conspicuous than is the case with its English followers such as Ingram or Ashley, it was no less present and is overlooked only because historism is erroneously connected with the historical method of the older historians, instead of with the views of Roscher, Hildebrandt and particularly Schmoller and his circle.

That human history deals with events or situations which are unique or singular when we consider all aspects which are relevant for the answer of a particular question we may ask about them, is, of course, not peculiar to human history. It is equally true of any attempt to explain a concrete phenomenon if we only take into account a sufficient number of aspects—or, to put it differently, so long as we do not deliberately select only such aspects of reality as fall within the sphere of that system of connected propositions which we call one theoretical science. If I watch and record the process by which a plot in my garden that I leave untouched for years is gradually covered with weeds, I am describing a process which in all its detail is no less unique than any event in human history. If I want to explain any particular configuration of different plants which may appear at any stage of that process, I can do so only by giving an account of all the relevant influences which have affected different parts of my plot at different times. I shall have to consider what I can find out about the differences of the soil in different parts of the plot, about differences in the radiation of the sun, of moisture, of the air-currents, etc., etc.; and in order to explain the effects of all these factors I shall have to use, apart from the knowledge of all these particular facts, various parts of the theory of physics, of chemistry, biology, meteorology, and so on. The result of all this will be the explanation of a particular phenomenon, but not a science of how garden plots go out of cultivation.

In an instance like this the particular sequence of events, their causes and consequences, will probably not be of sufficient general interest to make it worth while to write them up or to develop their study into a distinct discipline. But there are large fields of natural knowledge, represented by recognised disciplines, which in their methodological character are no different from this. In geography, e.g., and at least in a large part of geology and astronomy, we are mainly concerned with particular situations, either of the earth or of the universe; we aim at explaining a unique situation by showing how it has been produced by the operation of many forces subject to the general laws studied by the theoretical sciences. In the specific sense of a body of general rules in which the term "science" is often used[1] these disciplines are not "sciences", i.e., they are not theoretical sciences but endeavours to apply the laws found by the theoretical sciences to the explanation of particular "historical" situations.

The distinction between the search for generic principles and the explanation of concrete phenomena has thus no necessary connection with the distinction between the study of nature and the study of society. In both fields we need generalisations in order to explain concrete and unique events. Whenever we attempt to explain or understand a particular phenomenon we can do so only by recognising it or its parts as members of certain classes of phenomena, and the explanation of the particular phenomenon presupposes the existence of general rules.

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[1] It will be noted that this, still restricted, use of the term "science" (in the sense in which the Germans speak of *Gesetzeswissenschaft*) is wider than the even narrower sense in which its meaning is confined to the theoretical sciences of nature.

There are very good reasons, however, for a marked difference in emphasis, reasons why, generally speaking, in the natural sciences the search for general laws has the pride of place while their application to particular events is usually little discussed and of small general interest, while with social phenomena the explanation of the particular and unique situation is as important and often of much greater interest than any generalisations. In most natural sciences the particular situation or event is generally one of a very large number of similar events, which as particular events are only of local and temporary interest and scarcely worth public discussion (except as evidence of the truth of the general rule). The important thing for them is the general law applicable to all the recurrent events of a particular kind. In the social field, on the other hand, a particular or unique event is often of such general interest and at the same time so complex and so difficult to see in all its important aspects, that its explanation and discussion constitutes a major task requiring the whole energy of a specialist. We study here particular events because they have contributed to create the particular environment in which we live or because they are part of that environment. The creation and dissolution of the Roman Empire or the Crusades, the French Revolution or the Growth of Modern Industry are such unique complexes of events, which have helped to contribute the particular circumstances in which we live and whose explanation is therefore of great interest.

It is necessary, however, to consider briefly the logical nature of these singular or unique objects of study. Probably the majority of the many disputes and confusions which have arisen in this connection are due to the vagueness of the common notion of what can constitute *one* object of thought—and particularly to the misconception that the totality (i.e., all possible aspects) of a particular situation can ever constitute one single object of thought. We can here touch only on a very few of the logical problems which this belief raises.

The first point which we must remember is that, strictly speaking, *all* thought must be to some degree abstract. We have seen before that all perception of reality, including the simplest sensations, involves a classification of the object according to some property or properties. The same complex of phenomena which we may be able to discover within given temporal and spatial limits may in this sense be considered under many different aspects; and the principles according to which we classify or group the events may differ from each other not merely in one but in several different ways. The various theoretical sciences deal only with those aspects of the phenomena which can be fitted into a single body of connected propositions. It is necessary to emphasise that this is no less true of the theoretical sciences of nature than of the theoretical sciences of society, since an alleged tendency of the natural sciences to deal with the " whole " or the totality of the real things is often quoted by writers inclined to historism as a justification for doing the same in the social field.[1] Any discipline of knowledge, whether theoretical or historical, can, however, deal only with certain selected aspects of the real world; and in the theoretical sciences the principle of selection is the possibility of subsuming these aspects under a logically connected body of rules. The same thing may be for one science a pendulum, for another a lump of brass, and for a third a convex mirror. We have already seen that the fact that a pendulum possesses chemical and optical properties does not mean that in studying laws of pendulums we must study them by the methods of chemistry and optics—though when we apply these laws to a particular pendulum we may well have to take into account certain laws of chemistry or optics. Similarly, as has been pointed out, the fact that all social phenomena have physical properties does not mean that we must study them by the methods of the physical sciences.[2]

The selection of the aspects of a complex of phenomena which can be explained by means of a connected body of rules is, however, not the only method of selection or abstraction which the scientist will have to use. Where investigation is directed, not at establishing rules of general applicability, but at answering a particular question raised by the events in the world about him, he will have to select those features that are relevant to the particular question. The important point, however, is that he still must select a limited number from the infinite variety of phenomena which he can find at the given time and place. We may, in such cases, sometimes speak as if he considered the " whole " situation as he finds it. But what we mean is not the inexhaustible totality of everything that can be observed within certain spatio-temporal limits, but certain features thought to be relevant to the question asked. If I ask why the weeds in my garden have grown in this particular pattern no single theoretical science will provide the answer. This, however, does not mean that to answer we must know everything that can be known about the space-time interval in which the phenomenon occurred. While the question we ask designates the phenomena to be explained, it is only by means of the laws of the theoretical sciences that we are able to select the other phenomena which are relevant for its explanation. The object of scientific study is never the totality of all the phenomena observable at a given time and place, but always only certain selected aspects : and according to the question we ask the same spatio-temporal situation may become any number of different objects of study. The human mind indeed can never grasp a " whole " in the sense of all the different aspects of a real situation.

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[1] Cf. e.g., E. F. M. Durbin, " Methods of Research—A Plea for Co-operation in the Social Sciences," *Economic Journal*, June, 1938, p. 191, where the writer argues that in the social sciences " unlike the natural sciences, our subdivisions are largely (though not entirely) *abstractions from* reality rather than *sections* of reality " and asserts of the natural sciences that " in all these cases the object of study are real independent objects and groups. They are not aspects of something complex. They are real things ". How this can be really asserted, e.g., of Crystallography (one of Mr. Durbin's examples) is difficult to comprehend.—This argument has been extremely popular with the members of the German historical school in economics, though, it should be added, Mr. Durbin is probably entirely unaware how closely his whole attitude resembles that of the *Kathedersozialisten* of that school.

[2] It is significant that all of H. Rickert's well-known work on the differences between the *Naturwissenschaften* and *Kulturwissenschaften* is based on the assertion that since all phenomena which we can observe are physical phenomena, all generalising (theoretical) science must be physical science.

The application of these considerations to the phenomena of human history leads to very important consequences. It means nothing less than that a historical process or period is never a single definite object of thought but becomes this only by the question we ask about it ; and that according to the question we ask, what we are accustomed to regard as a single historical event can become any number of different objects of thought.

It is confusion on this point which is mainly responsible for the doctrine now so very much in vogue that all historical knowledge is necessarily relative, determined by our " standpoint " and bound to change with the lapse of time.[1] This view is a natural consequence of the belief that the commonly used names for historical periods or complexes of events, such as " the Napoleonic Wars ", or " France during the Revolution ", or " the Commonwealth Period ", stand for definitely given objects, unique individuals[2] which are given to us in the same manner as is the case with the natural units in which biological specimens or planets are given to us. Those names of historical phenomena define in fact little more than a period and a place and there is scarcely a limit to the number of different questions which we can ask about events which occurred during the period and within the region to which they refer. It is, however, only the question that we ask which will define our object ; and there are, of course, many reasons why at different times people will ask different questions about the same period.[3] But this does not mean that history will at different times and on the basis of the same information give different answers to the same question. This alone, however, would entitle us to assert that historical knowledge is relative. The kernel of truth in the assertion about the relativity of historical knowledge is that historians will at different times be interested in different objects, but not that they will necessarily hold different views about the same object.

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[1] For a good survey of the modern theories of historical relativism see M. Mandelbaum. *The Problem of Historical Knowledge*, New York, 1938.

[2] Cf. below, p. 57, footnote.

[3] It is not possible to pursue further here the interesting question of the reasons which make the historian ask particular questions and which make him ask at different times different questions about the same period. We ought, however, perhaps briefly to refer to one view which has exercised wide influence, since it claims application not only to history but to all *Kulturwissenschaften*. It is Rickert's contention that the social sciences, to which, according to him, the historical method is alone appropriate, select their object exclusively with reference to certain values with respect to which they are important. Unless by " value consideration " (*Wertbezogenheit*) any kind of practical interest in a problem is meant so that this concept would include the reasons which make us, say, study the geology of Cumberland, this is certainly not necessarily the case. If, merely to indulge my taste in detective work, I try to find out why in the year x Mr. N. has been elected mayor of Cambridge, this is no less historical work though no known value may have been affected by the fact that Mr. N. rather than somebody else has been elected. It is not the reason why we are interested in a problem but the character of the problem which makes it a historical problem.

We must dwell a little longer on the nature of the "wholes" which the historian studies, though much of what we have to say is merely an application of what has been said before about the "wholes" which some authors regard as objects of theoretical generalisations. What we said then is just as true of the wholes which the historian studies. They are never given to him as wholes, but always reconstructed from their elements which alone can be directly perceived. Whether he speaks about the government that existed or the trade that was carried on, the army that moved, or the knowledge that was preserved or disseminated, he is never referring to a constant collection of physical attributes that can be directly observed, but always to a system of relationships between some of the observed elements which can merely be inferred. Words like government or trade or army or knowledge do not stand for single observable things but for structures of relationships which can be described only in terms of a schematic representation or "theory" of the persistent system of relationships between the ever-changing elements.[1] These "wholes", in other words, do not exist for us apart from the theory by which we constitute them, apart from the mental technique by which we can reconstruct the connections between the observed elements and follow up the implications of this particular combination.

The place of theory in historical knowledge is thus in forming or constituting the wholes to which history refers, it is prior to these wholes which are not visible except by following up the system of relations which connects the parts. The generalisations of theory, however, do not refer, and cannot refer, as has been mistakenly believed by the older historians (who for that reason opposed theory), to the concrete wholes, the particular constellations of the elements, with which history is concerned. The models of "wholes", or structural connections, which theory provides ready-made for the historian to use (though even these are not the given elements about which theory generalises but the results of theoretical activity), are not identical with the "wholes" which the historian considers. The models provided by any one theoretical science of society consist necessarily of elements of one kind, elements which are selected because their connection can be explained by a coherent body of principles and not because they help to answer a particular question about concrete phenomena. For the latter purpose the historian will regularly have to use generalisations belonging to different theoretical spheres. His work, thus, as in all attempts to explain particular phenomena, presupposes theory; it is, as is all thinking about concrete phenomena, an application of generic concepts to the explanation of particular phenomena.

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[1] It does not alter the essential fact that the theorising will usually already have been done for the historian by his source which in reporting the "facts" will use such terms as "state" or "town" which cannot be defined by physical characteristics but which refer to a complex of relationships which, made explicit, is a "theory" of the subject.

If the dependence of the historical study of social phenomena on theory is not always recognised this is mainly due to the very simple nature of the majority of theoretical schemes which the historian will employ and which brings it about that there will be no dispute about the conclusions reached by their help and little awareness that he has used theoretical reasoning at all. But this does not alter the fact that in their methodological character and validity the concepts of social phenomena which the historian has to employ are essentially of the same kind as the more elaborate models produced by the systematic social sciences. All the unique objects of history which he studies are in fact either constant patterns or relations, or repetitive processes in which the elements are of a generic character. When the historian speaks of a State or a battle, a town or a market, these words cover coherent structures of individual phenomena which we can comprehend only by understanding the intentions of the acting individuals. If the historian speaks of a certain system, say the feudal system, persisting over a period of time, he means that a certain pattern of relationships continued, a certain type of actions were regularly repeated, structures whose connection he can understand only by mental reproduction of the individual attitudes of which they were made up. The unique wholes which the historian studies, in short, are not given to him as individuals,[1] natural units of which he can find out by observation which features belong to them, but constructions made by the kind of technique that is systematically developed by the theoretical sciences of society. Whether he endeavours to give a genetic account of how a particular institution arose, or a descriptive account of how it functioned, he cannot do so except by a combination of generic considerations applying to the elements from which the unique situation is composed. Though in his work of reconstruction he cannot use any elements except those he empirically finds, not observation but only the "theoretical" work of reconstruction can tell him which among those that he can find are part of a connected whole.

Theoretical and historical work are thus logically distinct but complementary activities. If their task is rightly understood, there can be no conflict between them. And though they have distinct tasks, neither is of much use without the other. But this does not alter the fact that neither can theory be historical nor history theoretical. Though the general is of interest only because it explains the particular and though the particular can be explained only in generic terms, the particular can never be the general and the general never the particular. The unfortunate misunderstandings that have arisen between historians and theorists are largely due to the name " historical school " which has been usurped by the mongrel view better described as historism and which is indeed neither history nor theory.

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[1] The confusion which reigns in this field has evidently been assisted by a purely verbal confusion apt to arise in German in which most of the discussions of this problem have been conducted. In German the singular or unique is called the *Individuelle*, which almost inevitably calls forth a misleading association with the term for the individual (*Individuum*). Now, individual is the term which we employ to describe those natural units which in the physical world our senses enable us to single out from the environment as connected wholes. Individuals in this sense, whether human individuals or animals or plants, or stones, mountains or stars, are constant collections of sense attributes which, either because the whole complex can move together in space relatively to its environment, or for cognate reasons, our senses spontaneously single out as connected wholes. But this is precisely what the objects of history are not. Though singular (*individuell*), as the individual is, they are not definite individuals in the sense in which this term is applied to natural objects. They are not given to us as wholes but only found to be wholes.

## X

The naïve view which regards the complexes which history studies as given wholes naturally leads to the belief that their observation can reveal " laws " of the development of these wholes. This belief is one of the most characteristic features of that scientistic history which under the name of historism was trying to find an empirical basis for a theory of history or (using the term philosophy in its old sense equivalent to " theory ") a " philosophy of history " and to establish necessary successions of definite " stages " or " phases ", " systems " or " styles " which followed each other in historical development. This view on the one hand endeavours to find laws where in the nature of the case they cannot be found, in the succession of the unique and singular historical phenomena, and on the other hand denies the possibility of the kind of theory which alone can help us to understand unique wholes, the theory which shows the different ways in which the familiar elements can be combined to produce the unique combinations we find in the real world. The empiricist prejudice thus led to an inversion of the only procedure by which we can comprehend historical wholes, their reconstruction from the parts ; it induced scholars to treat as if they were objective facts vague conceptions of wholes which were merely intuitively comprehended ; and it finally produced the view that the elements which are the only thing that we can directly comprehend and from which we must reconstruct the wholes could, on the contrary, be understood only from the whole, which had to be known before we could understand the elements.

The belief that human history, which is the result of the interaction of innumerable human minds, must yet be subject to simple laws accessible to human minds is now so widely held that few people are at all aware what an astonishing claim it really implies. Instead of working patiently at the humble task of rebuilding from the directly known elements the complex and unique structures which we find in the world, and at tracing from the changes in the relations between the elements the changes in the wholes, the authors of these pseudo-theories of history pretend to be able to arrive by a kind of mental short cut at a direct insight into the laws of succession of the immediately apprehended wholes. However doubtful their status, these theories of development have achieved a hold on public imagination much greater than any of the results of genuine systematic study. " Philosophies " or " theories " of history (or " historical theories ") have indeed become the characteristic feature, the " darling vice "[1] of the 19th century. From Hegel and Comte, and particularly Marx, down to Sombart and Spengler these spurious theories came to be regarded as representative results of social science; and through the belief that one kind of " system " must as a matter of historical necessity be superseded by a new and different " system " they have even exercised a profound influence on social evolution. This they achieved mainly because they looked like the kind of laws which the natural sciences produced; and in an age when these sciences set the standard by which all intellectual effort was measured, the claim of these theories of history to be able to predict future developments was regarded as evidence of their pre-eminently scientific character. Though merely one among many characteristic 19th century products of this kind, Marxism more than any of the others has become the vehicle through which this result of scientism has gained so wide an influence that many of its opponents equally with its adherents are thinking in its terms.

Apart from setting up a new ideal this development had, however, also the negative effect of discrediting the existing theory on which past understanding of social phenomena had been based. Since it was supposed that we could directly observe the changes in the whole of society or of any particular changed social phenomenon, and that everything within the whole must necessarily change with it, it was concluded that there could be no timeless generalisations about the elements from which these wholes were built up, no universal theories about the ways in which they might be combined into wholes. All social theory, it was said, was necessarily historical, *zeitgebunden*, true only of particular historical " phases " or " systems ".

All concepts of individual phenomena, according to this strict historism, are to be regarded as merely historical categories, valid only in a particular historical context. A price in the 12th century or a monopoly in the Egypt of 400 B.C., it is argued, is not the same " thing " as a price or a monopoly to-day, and any attempt to explain that price or the policy of that monopolist by the same theory which we would use to explain a price or a monopoly of to-day is therefore vain and bound to fail. This argument is based on a complete misapprehension of the function of theory. Of course, if we ask why a particular price was charged at a particular date, or why a monopolist then acted in a particular manner, this is a historical question which cannot be fully answered by any one theoretical discipline; to answer it we must take into account the particular circumstances of time and place. But this does not mean that we must not, in selecting the factors relevant to the explanation of the particular price, etc., use precisely the same theoretical reasoning as we would with regard to a price of to-day.

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[1] L. Brunschvicg, in *Philosophy and History, Essays presented to E. Cassirer*, ed. by R. Klibansky and H. J. Paton, Oxford, 1936, p. 30.

What this contention overlooks is that " price " or " monopoly " are not names for definite " things ", fixed collections of physical attributes which we recognise by some of these attributes as members of the same class and whose further attributes we ascertain by observation ; but that they are objects which can be defined only in terms of certain relations between human beings and which cannot possess any attributes except those which follow from the relations by which they are defined. They can be recognised by us as prices or monopolies only because, and in so far as, we can recognise these individual attitudes and from these as elements form the structural pattern which we call a price or monopoly. Of course the " whole " situation, or even the "whole" of the men who act, will greatly differ from place to place and from time to time. But it is nothing but our capacity to recognise the familiar elements from which the unique situation is made up which enables us to attach any meaning to the phenomena. Either we cannot thus recognise the meaning of the individual actions, they are nothing but physical facts to us, the handing over of certain material things, etc., or we must place them in the mental categories familiar to us but not definable in physical terms. If the first contention were true, however, this would mean that we could not know the facts of the past at all because in that case we could not understand the documents from which we derive all knowledge of them.[1]

Consistently pursued historism necessarily leads to the view that the human mind is itself variable and that not only are most or all manifestations of the human mind unintelligible to us apart from their historical setting, but that from our knowledge of how the whole situations succeed each other we can learn to recognise the laws according to which the human mind changes, and that it is the knowledge of these laws which alone puts us in a position to understand any particular manifestation of the human mind. Historism, because of its refusal to recognise a compositive theory of universal applicability, unable to see how different configurations of the same elements may produce altogether different complexes, and unable, for the same reason, to comprehend how the wholes can ever be anything but what the human mind consciously designed, was bound to seek the cause of the changes in the social structures in changes of the human mind itself—changes which it claims to understand and explain from changes in the directly apprehended wholes. From the extreme assertion of some sociologists that logic itself is variable, and the belief in the "pre-logical" character of the thinking of primitive people, to the more sophisticated contentions of the modern " sociology of knowledge ", this approach has become one of the most characteristic features of modern sociology. It has raised the old question of the " constancy of the human mind " in a more radical form than has ever been done before.

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[1] Cf. C. V. Langlois and C. Seignobos, *Introduction to the Study of History*, transl. by G. G. Berry, London, 1898, p. 222 : " If former humanity did not resemble humanity of to-day, documents would be unintelligible ".

This phrase is, of course, so vague that any dispute about it without giving it further precision is not likely to be profitable. That not only any human individual in its historically given complexity, but also certain types predominant in particular ages or localities, differ in significant respects from other individuals or types is, of course, beyond dispute. But this does not alter the fact that in order that we should be able to recognise or understand them at all as human beings or minds there must be certain invariable features present. We cannot recognise " mind " in the abstract. When we speak of mind what we mean is that certain phenomena can be successfully interpreted on the analogy of our own mind, that the use of the familiar categories of our own thinking provides a satisfactory working explanation of what we observe. But this means that to recognise something as mind is to recognise it as something similar to our own mind, and that the possibility of recognising mind is limited to what is similar to our own mind. To speak of a mind with a structure fundamentally different from our own, or to claim that we can observe changes in the basic structure of the human mind is not only to claim what is impossible ; it is a meaningless statement. Whether the human mind is constant can never become a problem—because to recognise mind cannot mean anything but to recognise something as operating in the same way as our own thinking.

To recognise the existence of a mind always implies that we add something to what we perceive with our senses, that we interpret the phenomena in the light of our own mind, or find that they fit into the ready pattern of our own thinking. This kind of interpretation of human actions may not be always successful, and, what is even more embarrassing, we may never be absolutely certain that it is correct in any particular case ; all we know is that it works in the overwhelming number of cases. Yet it is the only basis on which we ever understand what we call other people's intentions or the meaning of their actions ; and certainly the only basis of all our historical knowledge since this is all derived from the understanding of signs or documents. As we pass from men of our own kind to different types of beings we may, of course, find that what we can thus understand becomes less and less. And we cannot exclude the possibility that one day we may find beings who, though perhaps physically resembling men, behave in a way which is entirely unintelligible to us. With regard to them we should indeed be reduced to the " objective " study which the behaviourists want us to adopt toward men in general. But there would be no sense in ascribing to these beings a mind different from our own. We should know nothing of them which we could call mind, we should indeed know nothing about them but physical facts. Any interpretation of their actions in terms of such categories as intention or purpose, sensation or will, would be meaningless. A mind about which we can intelligibly speak must be like our own.

The whole idea of the variability of the human mind is a direct result of the erroneous belief that mind is an object which we observe as we observe physical facts. The sole difference, however, between mind and physical objects, which entitles us to speak of mind at all, is precisely that wherever we speak of mind we interpret what we observe in terms of categories which we know only because they are the categories in which our own mind runs. There is nothing paradoxical in the claim that all mind must run in terms of certain universal categories of thought, because where we speak of mind this means that we can successfully interpret what we observe by arranging it in these categories. And anything which can be comprehended through our understanding of other minds, anything which we recognise as specifically human, must be comprehensible in terms of these categories.

Through this theory of the variability of the human mind, to which the consistent development of historism leads, it cuts, in fact, the ground under its own feet: it is led to the self-contradictory position of generalising about facts which, if the theory were true, could not be known. If the human mind were really variable so that, as the extreme adherents of historism assert, we could not directly understand what people of other ages meant by a particular statement, history would be inaccessible to us. The wholes from which we are supposed to understand the elements would never be visible to us. And even if we disregard this fundamental difficulty created by the impossibility of understanding the documents from which we derive all historical knowledge, without first understanding the individual actions and intentions the historian could never combine them into wholes and never explicitly state what these wholes are. He would, as indeed is true of so many of the adherents of historism, be reduced to talking about "wholes" which are intuitively comprehended, to making uncertain and vague generalisations about "styles" or "systems" whose character could not be precisely defined.

It follows indeed from the nature of the evidence on which all our historical knowledge is based that history can never carry us beyond the stage where we can understand the working of the minds of the acting people because they are similar to our own. Where we cease to understand, where we can no longer recognise categories of thought similar to those in terms of which we think, history ceases to be human history. And precisely at that point, and only at that point, do the general theories of the social sciences cease to be valid. Since history and social theory are based on the same knowledge of the working of the human mind, the same capacity to understand other people, their range and scope is necessarily co-terminus. Particular propositions of social theory may have no application at certain times, because the combination of elements to which they refer to do not occur.[1] But they remain nevertheless true. There can be no different theories for different ages, though at some times certain parts and at others different parts of the same body of theory may be required to explain the observed facts, just as, e.g., generalisations about the effect of very low temperatures on vegetation may be irrelevant in the tropics but still true. Any true theoretical statement of the social sciences will cease to be valid only where history ceases to be human history. If we conceive of somebody observing and recording the doings of another race, unintelligible to him and to us, his records would be in a sense history as, e.g., the history of an ant-heap. Such history would have to be written in purely objective, physical terms. It would be the sort of history which corresponds to the positivist ideal, such as the proverbial observer from another planet might write of the human race. But such history could not help us to understand any of the events recorded by it in the sense in which we understand human history.

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[1] Cf. W. Eucken, *Grundlagen der Nationalökonomie*, 1940, pp. 203–205.

When we speak of man we necessarily imply the presence of certain familiar mental categories. It is not the lumps of flesh of a certain shape which we mean, nor any units performing definite functions which we could define in physical terms. The completely insane, none of whose actions we can understand, is not a man to us—he could not figure in human history except as the object of other people's acting and thinking. When we speak of man we refer to one whose actions we can understand. As old Democritus said

*ἄνθρωπος ἔστιν ο πάντες ἴδμεν.*[1]

*(To be concluded.)*

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[1] "Man is what is known to all." Cf. H. Diehls, *Die Fragmente der Vorsokratiker*, 4th ed., Berlin, 1922; Democritus, Fragment No. 165, Vol. II, p. 94. I owe the reference to Democritus in this connection to Professor Alexander Rüstow.

# Note on Mr. Barna's Valuation of Stocks[1]

### By A. L. Bowley

Some explanation is due to Mr. Barna, since the passage in *National Income* to which he refers on pp. 355–6 was misleading, owing to some slip between the first writing of it and the final print.

The notation used is $p_0$, $p_1$ average current values of stocks, $q_0$, $q_1$, at the beginning and end of a year; $p'_0$, $p'_1$ the average prices at which they were purchased; $q$ the quantity consumed and $bq$ the quantity bought during the year; $p_m$ the average price of the materials during the year.

$Z = p_m(q_1 - q_0)$, the value required according to the definition of income adopted.
$X = p_1q_1 - p_0q_0$, manufacturer's valuation at current prices.
$Y' = p_1'q_1 - p_0'q_0$, " " " cost price.
$X' = p_1q_1 - p_0'q_0$, valuation at cost at beginning, current price at end of year.
$Y = p_1'q_1 - p_0q_0$, valuation at current price at beginning, cost at end of year.

In my formula under question I was concerned only with the *increase* in value, and I intended to take the case where the manufacturer used the same method of valuation at the beginning and end of the year, so that only the excess of cost of purchase over cost of consumption was concerned; but $p_0q_0$ was printed for the cost of the original stock instead of $p_0'q_0$. Since the same quantity was subtracted, my subsequent analysis was not affected. In other words, I aimed at Mr. Barna's $Y'$ not $Y$. Most of the criticism on pp. 355–6 is therefore irrelevant, except that I had not explicitly stated that I assumed the same rates of change over the whole period involved.

Mr. Barna's method of evaluating purchasing price (p. 356) is, I think, more plausible than mine. I assumed that every bale used for consumption was drawn from goods purchased $12m$ months earlier, an artificial and inelastic assumption when the rate of purchase is changing. Mr. Barna assumes that the material that has been longest in stock is used first. My assumption is perhaps more appropriate to a heterogeneous, his to a homogeneous stock.[2]

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[1] Economica, November, 1942, pp. 349–358.

[2] With reference to Mr. Barna's note at the bottom of p. 356: If the sign of $X - Y$ in my Table (*National Income*, p. 121) is negative, the stock at the end of the year is insufficient to carry on the former rate of consumption. The cases taken are the limits when the stock has run down to zero at the end of the year.

Mr. Barna interprets the rule of the Income Tax Commissioners or the practice of manufacturers in the sense that the manufacturer must keep to his former valuation of stock at the beginning of the year, whether he chooses current or cost value at its end. No doubt if he has returned $p_0 q_0$ he may not be able readily to evaluate $p_0' q_0$; but if he has chosen $p_0' q_0$, it is probable that his books will also show $p_0 q_0$, and he can pass from $Y'$ to $X$, though not easily from $X$ to $Y'$, if the authorities allow him to do so. However that may be, it is interesting to examine the difference between $X$ and $Y'$, appropriate to the case when the same choice has been made in two or more years.

Using Mr. Barna's method of valuing stocks (p. 356), we may take for simplicity the special case when prices are rising or falling and stocks increasing or diminishing regularly over the whole period in question. Take $p_0 = 1$ and the consumption in the year $q = 1$, thereby not losing generality, and write $p_1 = p_0(1 + r) = 1 + r$; $p_m = \frac{1}{2}(p_1 + p_0) = 1 + \frac{r}{2}$; $q_m = \frac{1}{2}(q_1 + q_0)$; purchases $= bq = b$. Then $q_1 = q_0 + b - 1$. It is then found that

$$Z - X = -rq_m, \quad Z - Y' = -\frac{1}{b} rq_m, \quad Z - X' = -r\left(q_m + \frac{1}{2b} q_0^2\right),$$

$$Z - Y = -r\left(q_m - \frac{1}{2b} q_1^2\right).$$

If prices are rising, and stock increasing, then $b > 1$, and

$$X' > X > Y' > Y.$$

But if $b < 1$, $X' > Y' > X > Y$.

It follows that if prices and stocks are increasing over a series of years, the manufacturer should choose $Y'$, but if prices are rising and stocks decreasing he should choose $X$.[1]

The direction of inequality is reversed if prices are falling. If data were available the conditions could be worked out on less rigid hypotheses.

This result differs from Mr. Clark's, Mr. Maizel's and Mr. Barna's, which also differ from each other.

Unfortunately it is precisely in those years when prices or purchases are rising or falling rapidly, and the adjustments would be considerable, that any assumptions of regularity of movements of prices, purchases or consumption are least justified, and the method of averaging changes over wide industrial groups least plausible.

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[1] Such figures as those in Col. 3, p. 351 of Mr. Barna's article, show whether $b$ is greater or less than 1, but do not show the magnitude of the difference.

# Measurements of the Degree of Monopoly: a Note

### By Joe S. Bain

In his recent article on the degree of monopoly, Mr. Rothschild offers a novel alternative to Lerner's measure of monopoly power. Whereas Mr. Lerner suggested that monopoly power be measured by the discrepancy between price and marginal cost (or in equilibrium as the inverse of the elasticity of the seller's demand curve),[1] and I suggested that it be measured by the rate of profit,[2] Mr. Rothschild would measure it as the ratio of the slopes of the seller's "genus" ($D D'$) and "species" ($d d'$) demand curves.[3] This third definition is formally quite acceptable, and there is certainly no objection to measuring this additional aspect of a seller's market position. Concerning the usefulness of each of the three measures, however, I should like to make two comments.

First, the fact that sloping demand curves have become the vogue in contemporary price theory does not imply that measurements of departures from the competitive norm should always emphasise these slopes as the *quæsitum* of investigation, nor that these slopes are the most significant phases of other than purely competitive equilibrium market situations. There are two principal aspects of such non-competitive situations—the relationship of price to marginal cost and the relationship of price to average cost. The first is significant because of its implications for the allocation of factors of production. Mr. Lerner's measure, essentially one of the discrepancy between price and marginal cost, is relevant if we are interested in the effects of "monopoly" on allocation. The second is significant because of its influence directly on the functional distribution of income, and indirectly on the propensity to consume, the level of employment, etc. My measure of price-"average cost"[4] relationships is relevant if we are interested in the latter effects of departures from competitive equilibrium. *It is inappropriate to judge either of these measurements according to its properties as an approximation to the other, since they are designed to determine substantially different things.* The fact that Lerner's measure isolates the effect of monopoly (defined as a slope of the seller's demand curve) whereas mine lumps together the effects of monopoly, monopsony, and impediment to entry is merely an indication of a clear difference in our purposes. It does not follow, therefore, as Mr. Rothschild holds, that the profit rate measure is good in certain cases (pure competition with impeded entry) and the Lerner measure good in others. Each is good in all situations, but for different purposes. Whether one or the other is a better measure of "monopoly power" is a terminological issue.

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[1] A. P. Lerner, "The Concept of Monopoly and the Measurement of Monopoly Power," *Review of Economic Studies*, Vol. I, pp. 157–175.

[2] The rate of profit being defined as the difference between total revenue and total "ideal" or competitive cost, as a ratio to competitive investment. These special definitions were made necessary by the ambiguity of ordinary definitions of cost. See "The Profit Rate as a Measure of Monopoly Power," *Quarterly Journal of Economics*, February, 1941, pp. 271–293.

[3] K. W. Rothschild, "The Degree of Monopoly," ECONOMICA, February, 1942, pp. 24–39. The "genus-species" terminology was suggested by M. A. Copeland ("Competing Products and Monopolistic Competition," *Quarterly Journal of Economics*, November, 1940, pp. 1–36).

[4] Where average cost is rectified to a certain opportunity level.

Mr. Rothschild's own measure concerns a third distinct aspect of monopoly situations, and like the two just discussed fares poorly if considered as an approximation to other measures. Since the "genus-species ratio" has no direct connection with price-cost relationships, moreover, its magnitude would be of secondary importance in investigations concerned with particular or general market results.

My second comment concerns Mr. Rothschild's position regarding the practical usefulness of various theoretical measures of departures from competitive equilibrium. On page 30 of his article he says: "If in theoretical work we feel the need for *measurement* we should forge a *tool* which will best suit our theoretical need and should not allow ourselves to be sidetracked by the consideration of any statistical material just because it is easily available . . . the formulation of a theoretically useful formula will often stimulate the production of those statistics which are needed for quantitative studies".[1] The first part of this statement seems to me to be acceptable only within limits. If Mr. Rothschild means that for purposes of abstract reasoning, where empirical verification is not contemplated, we may deal with purely conceptual measurements on the basis of their logical nicety alone, I would, of course, agree. If, however, he means by *measurement* an empirical process and by *tool* a practical weapon for assailing economic data, certain qualifications must be imposed. A measurement to be potentially useful empirically must be *practically ascertainable*, and a tool must be practically *usable*. Although it is not necessary that the requisite data be already ascertained, they must at least be practically susceptible of ascertainment. It is not sufficient that they be merely conceivably ascertainable if in fact they are never likely to be made available.[2]

Measurements which depend directly on the slopes or elasticities of sellers' demand curves must in general be judged practically unascertainable. In all cases other than pure competition, the seller's demand curve is a schedule of his expectations which exists only in his own consciousness. It seems improbable that a reliable quantitative approximation to such a schedule could ever be obtained. In short, if a seller's monopoly power is to be measured in terms of the *ex ante* demand schedules for his product and for his "market", we must admit first that his monopoly power is what he thinks it is, and second that neither he nor we will probably be able to find out exactly what he thinks. This difficulty of the non-ascertainability of demand curves is greatly reinforced in all cases of oligopoly, where the seller's subjective path to his demand curve is presumably tortuous.

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[1] Italics mine.

[2] See J. S. Bain, "Market Classifications in Modern Price Theory," *Quarterly Journal of Economics*, August, 1942, pp. 563–565.

Although Lerner's definition of monopoly power may be drawn in terms of the elasticity of the seller's demand curve, knowledge of this curve is not required in order to find an approximation to this measure. In place of the ratio between marginal revenue and price we may (either with or without assuming that marginal cost is implicitly equated to marginal revenue) take the ratio between marginal cost and price. Price being known, we need merely to find marginal cost of production at going output in order to obtain a quantitative approximation. Ascertainment of marginal cost may be practically possible where ascertainment of seller's demand and marginal revenue is not. Similarly, no knowledge of sellers' demand curves is required in order to find the ratio of price to average cost. Mr. Rothschild's measure, on the other hand, seems to demand a direct knowledge of these demand curves, and it is therefore doubtful that its invention will stimulate the production of the requisite statistics. Although it furnishes an extremely useful concept by which we may abbreviate the genus-species relationship suggested by Chamberlin, it is probably not susceptible of empirical ascertainment.

# A Further Note on the Degree of Monopoly

By K. W. Rothschild

THE editor of ECONOMICA was so kind as to let me see the above note, and I should like to take this opportunity to add a few remarks to Mr. Bain's comments.

Mr. Bain is, of course, perfectly right in pointing out that different measures of monopoly power may all be appropriate and have to be judged by the problems which we want to determine. This was indeed one of the main points of my article. I also agree with Mr. Bain that his measure of the rate of profit is highly significant with regard to the functional distribution of income, the level of employment, etc. His measure is essentially one of supernormal profits. As Mr. Bain says, it lumps together the effects of monopoly, monopsony, limit to entry, and, I suppose, also to some extent the effects of frictional forces in a competitive system, like ignorance and special degrees of uncertainty.[1] To lump all these factors together is perfectly appropriate for a great number of economic problems. But why call it a measure of monopoly power? It *is* a terminological issue. But a clear and unequivocal terminology may help to avoid unnecessary confusion.

In connection with Mr. Bain's second comment (with the main point of which I agree) a minor misunderstanding must be corrected. In my index of monopoly power it is not the subjective (*ex ante*) seller's demand curve that matters, but the objective (*ex post*) curve. If a seller in a perfectly competitive market *thinks* that he has a certain hold over the market, his degree of monopoly will still be zero, as he would soon find out if he acted on his erroneous belief.

But while we thus get rid of the ambiguities of a subjective approach, the difficulties of quantitative measurement remain. For we shall be able to get some idea of the shape of the objective seller's demand curve only if the firm in question has experimented in its price policy under circumstances which are roughly comparable. And this is a rare event in these days of price rigidities and price leadership.

Thus the objection that no quantitative value can be given to that index will be true for the great majority of cases. But will this not be necessarily true for most indices which try to measure the degree of monopoly ? To measure it we shall always need some intimate information about conditions within a firm—information as to sales, average or marginal cost, value or unused capacity of machinery, etc. ; that such information will be available on a sufficiently wide scale to render adequate quantitative measurements of the degree of monopoly possible is not very likely in an age in which autocracy and secrecy are predominant features of the industrial system.

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[1] See the interesting classification of profits in F. Machlup, "Competition, Pliopoly and Profit", ECONOMICA, May and August, 1942.

# Book Reviews

*Monetary Theory.* By GEORGE N. HALM. Philadelphia: The Blakiston Company. 1942. 347 pp. $3.50.

This is a courageous attempt in that most difficult field, the compilation of a text-book incorporating current controversies. Deliberately excluding any detailed survey of banking practice, it sets out to provide a systematic survey of monetary theory, conceived as the search for the means of economic stabilisation. Within its ambit there is little of importance that is not touched upon, and the average student should learn much, and have little to unlearn, from it.

The book is not, however, one in which it is very easy to find one's way about. Partly this is due to the inevitable difficulties of arrangement; partly to the fact that the reader brings to it a preconceived ideal of form and content, to which the book very naturally fails to correspond. Nevertheless there does appear to the present reviewer to be something a little erratic about Mr. Halm's steering.

His opening chapters follow accepted principles, the first seven deploying quite skilfully the concepts which are being prepared for use—*M*, *V*, *P*, *T* and so forth. In chapter 8 we first meet the notion of stabilisation, which recurs in chapters 10, 12 and 20. This seems rather a pity; it is perfectly true that the nature of the problem is progressively revealed, but there is something of anti-climax in its intermittent appearance. Chapters 9, 11 and 12 deal with metallic standards and chapters 13 to 15 with theories of interest. Chapter 16, the greater part of which seems a little lost so late in the book, discusses the capital market. Chapters 17 and 19 cover the trade cycle, chapter 18 the multiplier and allied concepts and chapter 20 monetary policy proper. Faced with this alarming array, the student would have been much assisted by a clear briefing in advance, and by firm signposting at the various turns in the route.

A rather more drastic pruning of the phraseology would also have helped to secure the final touch of clarity. There are many sentences which have to be read two or three times before their intention becomes apparent, especially to the student who in any case has much ado to follow an abstract argument. Elsewhere the discussion does not quite get home for lack of a clear synthesis—as for example in chapter 15, where Lord Keynes' metamorphosis is studied without the aid of Professor Hicks' microscope.

American text-books always have, for English readers, the handicap that the references are unfamiliar, but Mr. Halm shows himself catholic in his reading, and though American editions are frequently cited, most of the authors we consult are duly mentioned. There are one or two odd omissions—Böhm-Bawerk does not appear in the chapters on interest; Mr. Kahn is not mentioned in connection with the multiplier, nor Bresciani-Turroni on inflation.

But although the book does not, in these respects at least, quite live up to the panegyrics bestowed upon it on its dust-cover, it is none the less a workmanlike job. Every teacher of monetary theory will find it helpful, and to all but the dullest students it will bring a fuller conception of what the subject is all about.

J. K. HORSEFIELD.

*Men of Substance. A Study of the Thought of Two English Revolutionaries, Henry Parker and Henry Robinson.* By W. K. JORDAN. University of Chicago Press. 1942. x + 283 pp.

A reviewer for ECONOMICA will not be expected to deal much with the first 139 pp. of this interesting study in the seventeenth century. They are biographical—almost superfluously so, as when Mr. Jordan assures us (p. 25, n. 108) that "it remains, in establishing the background of Henry Parker's thought, to sketch the history of his family during the next century"—or concerned with religion. Parker (1604–1652) was an Erastian and, in some ways, a forerunner of Hobbes. Robinson (1605–? 1673), as Dr. W. A. Shaw wrote in the D.N.B. forty-five years ago, was "perhaps the first Englishman to enunciate with clearness the principle of liberty of conscience". Both are important, and from Mr. Jordan we learn more about both. It is not necessary either to discuss pp. 140–202, which deal with their political thought, beyond saying that Robinson's was slight and that Parker's, as has been indicated, is worth serious study.

It is pp. 203–258, 'social and economic thought', that concern us. Here it is Parker's turn to be "slight, unsystematic and undistinguished" (p. 206), so he can be passed over. The much more important Robinson was in fact rather better known to us already than Mr. Jordan's treatment might suggest. Besides the 3½ columns of the D.N.B., there are substantial extracts from his two chief economic pamphlets—*England's safety in trades encrease*, 1641, and *Certain proposals in order to the people's freedom*, 1652—in Dr. Shaw's *Select Tracts and Documents Illustrative of English Monetary History, 1626–1730*, 1896, reissued 1935. Mr. Jordan knows Dr. Shaw' work but does not tell us what of Robinson it contains.

Robinson was a merchant and a glorifier of merchants. He claimed for them a better social status and more political influence than either Charles or Oliver normally allowed. He was, in a very vague way, a freetrader. As a merchant, he lacked that 'fear of goods' which Heckscher attributes to 'mercantilism'; he wanted more trade, lower customs, and customs-free ports for re-export. But he also wanted to snatch the trade of the East Indies by force, and he was for a big navy to break the Dutch. Like many before him, he was all for the fisheries, for Lenten fare and 'fish and navy days' to encourage them; although on the religious side he had not much use for Lent. (As Mr. Jordan puts it, this hoary fisheries policy reads like Robinson's invention.) He was an East India Company man and he wanted wide powers for the Company, to be used aggressively. He also advocated an extension of similar corporate activity among merchants.

Dr. Shaw and the late R. D. Richards have called attention to Robinson's advocacy of a national bank; and Mr. Jordan adds considerably to our knowledge of his sustained activity in that cause. But I am puzzled by the reference on p. 236 to his "proposal for a central bank, legally empowered to issue paper money as well as private bills with the validity of currency, first systematically presented by him in 1641". I am puzzled because I find nothing in *England's safety* which can be exactly translated by "legally empowered to issue paper money". Robinson is advocating a bank where "payments may be made by assignation" [from customer to customer of course, as at Amsterdam] which is to be "a grand cash keeper for the whole nation". He also advocates easy transferability of bills. This may well cover bills of his bank, but he does not say so. True, he suggests that transferable bills "likewise may be in print"; but he adds that "in the same manner as the bank before spoken of" they would "add livelyhood unto trade". This seems to suggest two distinct sources of that "livelyhood". Dr. Shaw, no more than I, seems to have seen legally empowered issue: his extract—the sole relevant passage—contains nothing of the kind. Maybe Robinson proposed it in some other context; but Mr. Jordan gives no other reference.

J. H. Clapham.

*Review of the Trade of India in 1940–41.* 67th Issue. By T. E. Gregory, Economic Adviser, Government of India. Calcutta. 1942.

Professor Gregory's *Review of the Trade of India in 1939–40*, dealt with the transition from peace to war and with the consequential repercussions on Indian trade. The current issue of the *Review* takes the story one stage further, showing how—after a considerable period of depression and an alarming drop in the prices of India's principal agricultural staples, which marked the reaction from the unfulfilled "dream of war prosperity" just after the outbreak of war—the tide again turned, so that from August, 1940, onwards there was a marked increase in general economic activity, a revival in exports, and some improvement in the prices of most staple commodities (except jute and sugar).

The reaction from the "dream of war prosperity" was so severe, owing to the shortage of shipping and other transport problems, the loss of Continental markets, and the consequential low level of prices of India's chief exports, that India narrowly escaped a serious agrarian crisis. Professor Gregory attributes the ultimate recovery to the increased industrial activity in India itself (which naturally increased the demand for raw materials); to the marked increase of exports to Empire markets, which gradually replaced the lost European markets; and to the restoration of confidence when it became clear that the Central and Provincial Governments intended to assist primary producers. For instance, a scheme of war-risk insurance to cover stocks of commodities was introduced, the original restrictions on the export of certain commodities to neutral countries were relaxed, an exploratory mission (including Professor Gregory himself) was sent to U.S.A., several more Indian Trade Commissioners were appointed in various foreign countries, and several projects were inaugurated to assist the producers of particular products (e.g. groundnuts, jute and coffee). In general it can be said that by the end of the year under review India had succeeded in making the most urgent adjustments necessitated by the war, and that a new phase of unexampled industrial activity had started. This effected certain remarkable changes in India's foreign trade, in particular a marked increase in exports of manufactures accompanied by a decrease in their imports, and on the other hand an increase in the imports of raw materials coupled with a decrease in their exports. Changes in the direction of Indian trade have been no less marked, the elimination of the trade with enemy countries and the diminution in the trade with England being to a considerable extent counterbalanced by a remarkable increase in trade with other Empire countries and with the United States.

There is no general statistical black-out in India, with the result that Professor Gregory has been able to a considerable extent to follow the traditional arrangement of the *Review*, and to include a large series of useful and illuminating charts and statistical tables. He starts with an admirably succinct survey of world events and their effects on Indian trade. Here it has been impossible to include the usual illustrative charts and statistics relating to world movements, and Professor Gregory warns the reader that the significance even of available statistics—for instance, of prices—is seriously diminished, in the absence of free markets, but this unavoidable deficiency has been to some extent remedied by the inclusion of tables showing the Total and Defence Expenditure of the United Kingdom, U.S.A., the Dominions and India (with some estimates for Germany). It is interesting to notice that although India's net defence expenditure rose from Rs. 46.18 lakhs in 1938–9 to Rs. 84.13 lakhs in 1941–2 (i.e. Budget estimate), this is a very much smaller proportionate increase than in any of the other cases. After this general review Professor Gregory deals *seriatim* with India's principal imports and exports, with the direction of trade, the trade of the Maritime Indian States, the Frontier trade, and (finally) with India's balance of trade. For " security reasons ", however, certain information and tables have had to be omitted, and there is one really big gap in the relevant facts and figures : i.e. " exports do not include the value of stores exported abroad for defence purposes ", and, similarly (but this is statistically much less important), " imports do not take into account goods imported for Defence Services in India ". These omissions were, no doubt, unavoidable, but the inevitable result is that the *Review* concentrates largely on the course of trade in staple commodities (and in a few well-established large-scale industries) and gives little or no idea of the great progress that has undoubtedly been made since September, 1939, in the munitions and related industries. Many incidental references are, of course, made to war policy and administration, but a special section dealing with war controls and measures would have been not only interesting and valuable, but would have provided a valuable corrective to the otherwise one-sided and possibly misleading picture that is presented. This would not have revealed any " secrets ", as information of this type is already officially provided periodically by *Indian Information.*

VERA ANSTEY.

*Doors to Jobs.* By E. H. HUNTINGDON. University of California Press. Great Britain : Cambridge University Press. 1942. xviii + 454 pp. 21s.

This excellent book contains the results of a study of placement agencies in California, undertaken in 1938 under the auspices of the Heller Committee for Research in Social Economics at the University of California. A more useful piece of field work that body could not have decided upon. The method used for the collection of facts was by interviewing the managers or owners of employment agencies and representative users of labour ; 1,100 interviews were obtained by six research workers over a period of six months. All occupations are included (including agricultural and domestic work, and film studios), but the study was confined to representative areas within the State of California (including the two metropolitan areas of Los Angeles and San Francisco). No attempt was made to interview the " final consumer ", the job-seeking worker. No fewer than 236 different types of " agencies " were covered in this way, classified as follows : 26 Governmental, 60 Social Services and Fraternal, 65 Schools and Colleges, 66 Commercial fee-charging, 8 Trade Unions, 30 Employers' Associations. No comprehensive figures could be secured, however, to calculate the relative importance of these various methods of filling vacancies.

The results of the enquiry are, on the whole, depressing. The majority of jobs are filled in a hit-and-miss manner " at the gates ". There is an *embarras de richesses* of placement agencies resulting in overlapping and confusion. In Professor Huntingdon's words: " Almost any worker in search of a job was confronted by so many choices that he might easily find it more economical of time and money to tramp from factory gate to factory gate rather than from the door of one employment agency to another " (p. 407). Illegal and " chiselling " practices abound in the case of fee-charging agencies and especially the " labour contractors ", the worst of whom have reduced workers almost to a state of peonage. (For readers of *The Grapes of Wrath*, this is the economics of the novel; the chapter on labour contractors, by Mary Gorringe Luck, is especially good.) The State Employment Service struggles uphill against a mass of prejudices on the part of employers. The " relief unemployed " is handicapped by the suspicion of being a " down-and-out ". One must not forget, of course, that 1938 was a year of severe depression in America, and that the labour surplus of a depression is not a propitious time for successful work by State or other organised placing agencies. In fact, there is evidence in the book that since then the State Employment Service has gained a more secure foothold in the labour market. Even so, the impression one gets is that much improvement is needed both for effective labour control in a war economy—California is a centre of war production now—and for the reduction of " frictional unemployment " in times of peace.

The book brings home to one both the similarities and differences in the development of a State Employment Exchange system here and in the U.S.A. In some ways the conditions described are similar to ours. (It is interesting to speculate on the advantages which the British Employment Exchange system derived from being started before the 1914 war and not in the midst of post-1919 unemployment.) The cluttering up of the exchanges with relief work interfering with their placing work, the policy of not giving preference on " social grounds " (long-term unemployed, or men with large families), all these and others are common to both countries. The scenes describing the taking over of relief work by the State Service—which occurred during the year of enquiry, 1938—remind one strongly of what happened in this country on the Second Appointed Day when the U.A.B. took over the able-bodied unemployed from the P.A.C.'s. The Advisory Councils attached to the State Service do not seem to be any more effective than they are here.

But even more impressive than the similarities are the differences which we note. Only just over one-half of the persons registered at the Californian State Exchanges were in receipt of public money (which compares with almost 90 per cent. here); the total number of their placements with private employers was 170,000 only (and two-thirds of them non-industrial). The corresponding figure for this country—allowing for difference in numbers—would be about 1.2

millions; but the actual number of placings by British Exchanges in 1938 was over 2.5 millions. Public relations work is much more important in American Exchanges; the administrative details which were disclosed to the investigators would be jealously guarded as "trade secrets" by British Civil Servants. In consequence, the enquiry on the State Service is as searching and, in parts, as damning (such as in the matter of the "padding" of placing statistics) as could be imagined, in this country, only by a Select Committee or Royal Commission commanding full evidence. (By the way, can we be quite sure that there is no element of "padding" in our statistics of vacancies filled by the Employment Exchanges?) The differences also come out in all sorts of minor ways, from the greater importance of Universities as job-finding agencies (for their students "working their way through college") to the spectacle of the unemployed roaming the State in search of work, in their motor cars.

In assessing the results, the special conditions in California must not be forgotten. The seasonal nature of much of its employment, and the constant influx of immigrants (mainly "drought refugees") are the two main ones. In the year of enquiry, the annual influx was estimated at 100,000. One need only realise what a difference an annual influx of 700,000 (a corresponding ratio) would have made to the British labour market and the work of the Employment Exchanges, to stop oneself from drawing rash analogies.

In short, the book is a model of institutional research applied in its proper field and in the proper way.

H. W. Singer.

*Labour Conditions in War Contracts.* I.L.O. Series D. No. 23. 1942. 59 pp. 1s.

The Reports which are periodically published by the I.L.O., dealing with comparative industrial and social legislation, perform a valuable service in keeping the international aspects of such subjects before our minds at a time when the national point of view is likely to loom unduly large.

The present study is concerned with the special conditions relating to War contracts in three countries—Canada, Great Britain and the United States; the comparison is interesting, but it would have been even more interesting if Australia and New Zealand had been included.

It is also unfortunate that the Report was written and published before Great Britain produced the new Fair Wages Clause which now takes the place of the old 1909 Resolution in Government contracts.

The Report stresses that the methods adopted by Canada and U.S.A. have certain features in common which are absent from the British procedure. In the former countries the rates of wages and the hours of work are specified for all contracts in some industries, or for certain classes of contracts. In Great Britain "recognised" wages and hours are required to be observed, but are not specified in any particular contract. In Canada and U.S.A. again the division of authority between the Federal and the Provincial or State Governments adds to the complications of wartime labour policy.

All three countries have some machinery for enforcing a fair wage policy in peacetime and this has been considerably extended to meet wartime requirements.

In Canada a Fair Wages Clause for Government contracts has been in operation since 1900, but since the beginning of the war the Fair Wages policy has been administered by the National War Labour Board with its dependent Regional Boards working in co-operation with the Dominion and Provincial Departments of Labour. Under the Wartime Wages and Cost of Living Bonus Order, wages are stabilised at the November, 1941, level, plus an agreed bonus, and increases and decreases are forbidden except by permission of the National War Labour Board. The Provisions of this Order also include hours of work, welfare arrangements, the right of workers to organise in Trade Unions and the settlement of disputes through collective bargaining.

In Great Britain a Fair Wages Clause has been in operation since 1909—and has lately been revised to bring it into line with modern conditions; for the period of the war further provision for the regulation of working conditions is made by the Conditions of Employment and National Arbitration Orders, the Essential Works Orders and the Factories Orders. It is clear from the Report that Collective Bargaining and Trade Union organisation generally is more definitely recognised as an integral part of the industrial system in Great Britain than in Canada.

The position of the United States is governed by various Federal Statutes applying to Federal Government contracts, certain of the provisions of the "New Deal", and the National Labour Defense Advisory organisation. War contracts are generally required to contain provisions for the regulation of hours and conditions, machinery for the settlement of industrial disputes, and arrangements for the welfare of workers. Little explicit emphasis is laid on collective bargaining and Trade Union organisation, but in so far as contracts are subject to the National Labour Relations Act, these points are included. Apparently the Service Departments have power to refuse contracts where the requirements of the last-named Act and of the Fair Labour Standards Act are not complied with. A forty-hour week is laid down as the standard, but provision is made for increasing this where necessary; but the maximum working week should not, it is stated, exceed 48 hours "since weekly hours in excess of this standard have been demonstrated to result in decreased rather than increased production when continued for any length of time". It is interesting to compare this with the British maximum of a 55–60 hour week, which as a result of the investigation of the Industrial Health Research Board, is regarded as the optimum and which is usually laid down in the Ministry of Labour regulations.

The Report concludes with an examination of one or two of the problems involved in the drafting and administration of labour regulations in war contracts, such as the scope of such regulations, both as regards the subjects they should cover and the industries which they should embrace; the question of wage policy; and the part to be played in war contracts of general legislation, labour clauses and collective bargaining. The Report is descriptive rather than critical and constructive in character, and does not make much attempt to suggest a solution of these problems. Indeed the background and conditions of the three countries discussed are so different that it would be very difficult, on the material given, to provide a possible solution. But within its limits the Report is an exceedingly useful document and provides some valuable information which it would be difficult to obtain in any other way.

R. C. Chambers.

*America's Trade Equality Policy*. By Hugh O. Davis. American Council on Public Affairs. 1942. Paper edition. 137 pp. $2.50.

The object of Mr. Davis' book on *America's Trade Equality Policy* is to explain exhaustively the origin, development and various forms of the most-favoured-nation clause as it has appeared in the commercial treaties of the United States since the end of the 18th century. Indeed, a substantial portion of the text consists of quotations from the chief commercial treaties, illustrating the exact wording of the conditional or unconditional (as the case may be) forms of the clause, with comments on the factors determining changes in this wording and the interpretation placed thereon in the United States and by other countries. The historical chapters are thorough and painstaking, and give a more accurate and detailed picture of the evolution of the most-favoured-nation clause in the commercial diplomacy of the United States than any other work with which I am acquainted. Mr. Davis' main thesis is that equality of treatment has always been the fundamental objective of American policy, but that the conditional form of the most-favoured-nation clause, which on the whole prevailed up to 1923, although originally designed to promote such equality, in fact resulted in discrimination. The conclusion therefore emerges that there is no sound alternative to the unconditional most-favoured-nation policy promoted recently so forcibly by Mr. Hull.

On the other hand, Mr. Davis confines himself very rigidly to his somewhat narrow and formal approach to his subject, with the result that his book will be of interest and value mainly to the student of the diplomacy of international trade agreements, and not to economists, those interested in the actualities of trade, or the "general reader". The latter classes of potential readers will still find Mr. Tasca's *The Reciprocal Trade Policy of the United States* far more readable and illuminating. Mr. Davis makes little or no attempt to describe the actual nature of the trade affected by the treaties, nor the changes in trade engendered by them. He is, in fact, concerned not with the substance or trends of trade, but merely with the formal conditions under which trade concessions have been given, received or withheld by the United States.

I can think of no stronger corrective to the insularity of the typical Englishman than a perusal of this volume. We are apt to think of Mr. Hull's policy, inaugurated in 1934, as centring round, or at least culminating in, the Anglo-American Commercial Treaty of November, 1938. But, strange to relate, Mr. Davis not only fails to analyse the objectives, terms and importance of this latter treaty, with its far-reaching implications with regard to Imperial Preference, but he even omits the Treaty (although he includes that simultaneously concluded with Canada) from his list of the reciprocal trade agreements negotiated by Mr. Hull between 1934 and the autumn of 1939. The work contains no bibliography and no index.

VERA ANSTEY.

*The Analysis of Economic Time Series.* By H. T. DAVIS. Cowles Commission for Research in Economics. Indiana. 1941. xiv + 620 pp.

The theory of frequency distributions, the applications of which are of fundamental importance in various branches of natural science, has acquired a well-established position in mathematical statistics. In applying statistics to economics, time series are at least of equal importance but have been comparatively neglected. Apart from index numbers, which form the subject of a separate theory, only some elementary methods, if any, of analysing time series are usually considered in statistical textbooks. The author is the first to give in one volume an account of the more advanced work done on this subject and mostly scattered in original publications. The large number of references shows what a formidable task this has been.

The book is divided into twelve chapters, each of which forms a more or less self-contained essay on one of the aspects of the problem. Roughly speaking, they may be classified as one introductory chapter, a first part comprising Chapters II—VII which deals mainly with statistical problems, and a second part containing Chapters VIII—XII which concerns itself more with the economic side of the question.

Chapter I gives a historical introduction into the economic problems associated with time series, the statistical problems arising out of them and the methods devised for their solution. It gives a very clear account of the subject matter and, being easy to read, it may be very useful to readers for whom some of the arguments set out in the following chapters are rather too technical.

In the statistical part, methods to analyse the components of time-series, viz. trend, harmonic and random elements, and to relate one series to another, are discussed in detail. Most prominent space is given to harmonic analysis, the tool used to detect length and amplitude of cycles. In their main parts, Chapter II describes the mathematical foundation and statistical technique of the periodogram and Chapter V discusses the significance of harmonic elements so found, whilst Chapter VII illustrates the method by means of examples derived from mathematical functions and actual economic series. It is seen that the results of periodogram analysis must not be taken at face value, but that an additional study of the autocorrelation of the series is necessary. The basis for such a study is given in Chapter III, which also deals with serial correlation between different series, and in Chapter IV, in which the autocorrelations of successive differences and moving averages of random series are investigated. This throws some light upon the problem how far regular fluctuations in economic time series may be caused by accumulation of random elements. Chapter VI is devoted to the problem of fitting trends to the data and of analysing the residuals. It contains a description of the variate difference method, which has lately been much discussed and about which apparently the last word has not yet been spoken.

The economic part of the book is to some extent open to criticism. For example, the distribution of income, which forms the subject of Chapter IX, seems rather far-fetched in the framework of this book, and the significance of magnitudes like the concentration ratio of income or wealth is exaggerated; the suggestion that the value of this ratio has to lie between certain limits, if political disturbances are to be avoided, is clearly untenable. Similarly, the practical value of the equation of exchange, on which Chapter X is based, and consequently that of the quantity theory of money, is overestimated. The eighth and the main part of the twelfth chapter deal with theories of and evidence for business cycles; in the former chapter, the study of the relations of interest rates as well as building activity to other economic variables deserves attention. The most interesting chapter in this part, however, seems to be the eleventh, dealing with forecasts of stock prices. After having balanced successes and failures of actual forecasts, the author develops an original theory of the standard error of forecast, showing the difficulties encountered in practice and the weaknesses of theories on the behaviour of the stock market.

It is not easy to find one's way through this work, which is not a systematic textbook. To the serious student of the more advanced methods in economic statistics, however, it will prove an invaluable aid. In order to become familiar with the essence of one or the other method, a study of the numerous examples set out in the text will prove particularly helpful.

C. E. V. Leser.

*Bank for International Settlements.* Twelfth Annual Report. Basle. 1942. 226 pp.

Only eight of the 226 pages of this Report, covering the fiscal year April, 1941, to March, 1942, deal with the affairs of the Bank, and even these give little information about its day-to-day activities beyond referring to the management of its investments and to its work as banker to the International Red Cross.

The remainder of the Report provides a comprehensive and impartial summary of the financial affairs of a world at war. In each of the subjects covered—prices, exchange rates, international trade and indebtedness, budgets, the stock exchange and the like—the principal facts are summarised for each of a long series of countries. General comments are few and cautious, emphasising only those inescapable complexities which are so easily overlooked in the single-minded economy of war. Thus, a twenty-page analysis of price movements in thirty-eight countries ends with a single critical paragraph, pointing the moral that the successful pursuit, after peace returns, of " a greater stability in the general price level must not be taken to mean that substantial adjustments in relative prices may be avoided ".

It is fortunate, in the present dearth of statistical material, that the Bank's wide contacts give it access to the internal affairs of all countries alike, and that it commands the services of an economist able to weigh up the immense mass of facts so obtained. As a source-book for details of the financial expedients and consequences of the war this Report is invaluable. It is the more to be regretted that the absence alike of detailed contents, index, and consistency in the arrangement of the chapters makes it necessary for the enquirer to hunt up the facts he wants by a process of trial and error.

J. K. HORSEFIELD.

*Indian Economics.* Vol. II (Sixth, Revised Edition). By G. B. JATHAR and S. G. BERI. Oxford University Press. 1941. 657 pp. 9s.

Jathar and Beri's two-volume work on Indian Economics has long had both a wide circulation and a well-deserved reputation as a standard authority on its subject. The delay in the appearance of this new edition of Vol. II (the sixth edition of Vol. I appeared in 1939) has given the authors the opportunity of including much new and interesting material relating to the effects of the war on Indian economic life and on the economic policy of the Government. The authors are to be congratulated on their careful revision of all the chapters and on their amazing up-to-dateness. It is of great value, especially to English readers, in these days of rapid changes but delayed news, to have this information collected in readily available form. Interesting recent events and developments are too numerous to be even listed here, but include information with regard to industrial progress, official schemes for the training of war technicians, the findings of the Gregory-Meek trade mission to U.S.A., the war-time Budget, the repatriation of Indian debt, and the trade agreements with Burma and Great Britain.

The arrangement of the chapters (Industrialisation, Transport, Trade, Prices, etc.), although it has obvious advantages, does not permit of a chronological, systematic analysis of the effects of the war, but in any case 1941 was too early a date to set about this task. But the manner and spirit in which the work has been undertaken—clear exposition, impartial discussion and judicial commentary—can hardly be too highly praised.

VERA ANSTEY.

" *Vested Interests or Common Pool?* " By NICHOLAS E. H. DAVENPORT. Victor Gollancz, Ltd. 1942. 183 pp. 8s. 6d.

This book is a powerful, popularly written, and often vehemently polemical criticism of the British war effort. The principal thesis enunciated is that " vested interests " prevent the fulfilment of the conditions of total war. The " vested interests " are identified especially with the Federation of British Industries and the Trades Union Congress outside Parliament, and the Conservative Party (representing " the employers' unions ") and the Labour Party (representing the trade unions) inside Parliament. The two factions fight each other about the division of the spoils, but join as partners in " robbing " the community. " Whenever the trade unions have won a bargain they have raised no objection at all to the subsequent raising of prices. It is not, therefore, the policy of the trade unions to oppose the monopolistic trend of big business " (p. 45). Nor, it might have been added, has it been the policy of organised capital to weaken the protection which the trade union " standard rate " has afforded to monopoly profits.

Mr. Davenport describes the recent development of the new industrial feudalism, he mentions the chief capitalist combines and trade associations, and he lists their " rivals in crime " (p. 45), namely, the major trade unions (pp. 58–61). The extent to which the present controllers have been chosen from those whose previous business connections have been with the great vested interests is shown (pp. 173–5), and it is suggested that the controllers have not successfully overcome their " dual allegiance ". Having " spent their lives scrapping redundant plant and limiting their output in the interests of monopoly and scarcity . . . were they not psychologically incapable of operating the Government controls with the ruthlessness required of total war? " (p. 67).

The Churchill Government failed adequately to rectify the situation, for the " basis of the Coalition was a mutual understanding " between " big business " and the trade unions (p. 72). The " key jobs were farmed out to the Party leaders of the vested interests " (p. 73), and in that way the old order was preserved, " labour looking after labour and capital after capital " (p. 75). Mr. Bevin, representing the trade union officials who were anxious " to maintain their jobs and their usefulness to their men " (p. 73), " expected inflation " and " seemed rather to welcome it . . . He said bluntly that wages must keep on going up to meet the rise in prices " (p. 74) ; and war-time wage negotiations in fact " enabled the trade union officials to enhance their prestige and influence among the rank and file . . . And what success they had ! " (p. 85). The earnings of the average worker increased by 42 per cent. against a 29 per cent. increase in the cost of living.

Inevitably, therefore, the inflation spiral was set in motion. The opposition of the unions even prevented an effective compulsory savings scheme from being adopted, which might have done something to stem the demand for non-essential, non-war goods (p. 106). The result was not only avoidable inflation but the weakening of rationing and the rise of black markets (p. 108). Attempts to get back part of the excess purchasing power through taxation had adverse repercussions. Income tax imposed for the first time upon workers' earnings " lessened the incentive to work and reduced the output of the factories " (p. 101). These and other causes, it is argued, have led to both confusion and ill-will.

Mr. Davenport's remedy for all this, like his diagnosis, is very similar to that recently suggested by the reviewer.[1] By the " common pool " he means an income stabilisation scheme, the State leasing the assets of all firms, paying compensation rentals to the owners, and paying fixed incomes (Mr. Davenport calls them " wages ") to the workers. Incomes being thus secured, private interests in the restraint of production will, he holds, no longer exist. Moreover, the tendency to ever-expanding inflation can then be curbed. Briefly stated like this, the practicability of the suggestion can hardly be conveyed. But as a scheme for war it merits careful and immediate study, the more so because it has virtues which Mr. Davenport himself does not claim, even if he perceives them. The great productive benefit to be hoped for from the income security conferred through the common pool is not only that created by a sense of justice, but the fact that it will permit the free valuation of the services of capital assets and labour. For this will enable the rational application of such productive services among the diverse competing objectives which make up the unified end of war. In other words, the common pool will make unfettered entrepreneurship possible. Surely one most important reason why restrictionism has been tolerated has been that the effects upon the income structure of the revaluation of physical and human assets due to the substitution of total war demands for peace demands would have created distributive injustices too serious for contemplation. Does not Mr. Davenport attribute too much to the cupidity of man and too little to innocent (although, equally disastrous) economic blindness? Entrepreneurship, as the present writer's previous article pointed out, is " so cruelly efficient that its distributive repercussions would have meant loss of morale and political difficulties ".[1]

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[1] " War Demand, Entrepreneurship and the Distributive Scheme ", ECONOMICA, November, 1941. The reviewer's diagnosis appeared in " Economic Lessons of the Allied War Effort," *South African Journal of Economics*, September, 1940.

Possibly because he has not fully recognised the immensely important rôle of value in co-ordinated production, Mr. Davenport appears to regard the " compensation rentals " to be paid to property owners, and the so-called " basis wages " to be paid to workers, as costs. But in fact they are compensation, and they will be entirely unrelated to costs in the more free regime of the future, although they have been to some extent determined by costs in the restrained regime of the past. If they are treated as values, misdirected effort and waste will be inevitable.

The same failure to envisage clearly enough the function of value co-ordinated production may be responsible for apparent contradiction in the rousing rhetorical passages with which the book abounds. Whilst at times he seems to be making a passionate plea for the preservation of private enterprise, at others he is scathingly condemning the " profit motive ". Unfortunately, the term " profit motive " can be used to cover opposite things, and propaganda has not been slow to confuse these opposites. What may be called the " profit elimination system " (under which resources are drawn into the production of goods and services in respect of which a profit is being earned, until profits are no longer achievable in that field—because resources are more urgently demanded in other fields) is the direct reverse of what may be called the " profit protection system " (under which resources can be excluded from production in the interests of the maintenance of profits). It is the latter which Mr. Davenport's scheme really seeks to supersede; although private profits will not be retained and private losses will not be borne under his war-time plan. That is, whilst a different motivation will have to be relied upon (because of the distribution effects of full employment), there should still be complete recourse to the profits index.

As the essence of a plan for reconstruction, the notion of the common pool to guarantee income security during periods of revolutionary readjustment, is most attractive. Again, the reviewer himself has built on the same idea in a book just published.[2] But on the reconstruction issue, Mr. Davenport's treatment of the problem does not go far enough. For firstly, he has not tackled the problem of *incentive* under the diversified ends of peace conditions; and the biggest difficulties which arise under income security are those of incentive. Certainly these difficulties are not insoluble, but they must be faced.

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[1] *Ibid.*, p. 345.

[2] *Plan for Reconstruction.* Kegan Paul, Trench, Trubner & Co., Ltd. 1943.

Secondly, he has not suggested any solution of the problem of withheld capacity, and this is an equally crucial question. He assumes, but does not demonstrate, that State planning of investment, State management of investment industries, State control of finance and the nationalisation of land will somehow provide a solution. But surely the disorder and waste which he recognises as having been caused by " monopolism ", i.e. the concentration of economic power, is hardly remediable by a further and still more rigid concentration. He puts forward no suggestions for overcoming what has been called the " private ownership of government ". What would he do about the " pressure groups " ? Does he think that the controllers of finance would be permitted to ignore the interests of combines, rings and unions in the interests of the community ? He would apparently leave these " rivals in crime ", as he calls them, intact.

Although these fundamental questions are neither raised nor answered, we feel that the book under review contains a valuable diagnosis of the chief restraints on total war and a valuable suggestion for a plan of immediate reform. But as a contribution to the literature of reconstruction it simply puts forward an undeveloped idea. It is an idea which might well bear fruit and have far-reaching consequences. The notion of the " common pool ", if it implies the temporary separation of the productive and distributive schemes, is as important an idea as any that British statesmen should now be examining. Properly understood, it could be used as the means of providing income security whilst deep-rooted changes intended to achieve social justice and the fullest employment of men and machines are taking place. Mr. Davenport's vigorous and burningly sincere book will undoubtedly help to put this idea into circulation. We hope that it will receive the attention it deserves in high circles.

W. H. HUTT.

*What Have We To Defend?* By E. F. M. DURBIN. Routledge & Son, Ltd. 1942. 95 pp. 5s.

Mr. Durbin wields the pen of a pleasing and persuasive writer. He has written a readable, and in places an eloquent, little book.

The " we " in the title means the people of this island. It is of the British social tradition, not of the values common to the peoples of the United Nations, that Mr. Durbin writes. To some readers this will be a fundamental limitation—one is tempted to say even a perversion—in a book which professes to explain, to quote the publisher's blurb, why a " pacific international socialist feels it to be his clear duty to support the war ". The effect of this approach is to make the war essentially a conflict of *national* values. An international socialist, one would have thought, would support the war because he prizes certain principles of freedom and social justice above all else, and believes that the triumph of Hitlerism would mean the betrayal of those principles. It may be that the people of Britain have done more than most to give practical expression to those principles; but in a world as hag-ridden by national passions as ours, is this the aspect that we most want to stress? It is upon the principles themselves and upon them alone, wherever and by whomsoever they are upheld and defended, that the foundations of a sane and happy world can be laid.

Neither the virtues which Mr. Durbin so justly admires, nor the faults which he so rightly condemns are, in fact, a peculiar British monopoly. That is what makes these and all other generalisations about national character so irritating to those who have observed the lack of correspondence between state boundaries and personal characteristics. For what they are worth, however, here are the main generalisations which Mr. Durbin has to offer. Take the virtues first: the British are a tolerant people, and tolerant not from laziness but from real love of their neighbour; they do not lack the "military virtues" of courage and discipline which Mr. Durbin once despised but has now learned to admire; and they are intelligent. They have been leaders in literature and thought.

On the other side of the account four great national faults have to be chalked up. Our economic life is disfigured by unjustifiable extremes of wealth and poverty; we are snobbish; we are vandals; and we are unimaginative.

The more dangerous aspects of this method of interpreting the war in terms of national, rather than moral, values appear when Mr. Durbin leaves the British to write of the Germans. It is difficult not to be shocked when one finds a "pacific international socialist" writing of "a darkness in the German *soul*[1] that no light of kindliness or reason has yet pierced". To confound the hateful political institutions of contemporary Germany with a darkness of the German *soul* is—well it is to abandon science for obscurantism, and to hand the result on a platter to Lord Vansittart. Is this what Mr. Durbin wants?

What it comes to, I think, is this. Mr. Durbin may please and hearten some British readers who like to enjoy the portrayal of their own virtues (and vices). But a Chinese, a French, a Russian or a German international socialist would find the book infuriating.

BARBARA WOOTTON.

## SHORTER NOTICES

*Economic Planning: Its Aims and Implications.* By CLAUDE DAVID BALDWIN. The University of Illinois Press, Urbana, Ill. 1942. 188 pp. $2.50.

This is a useful and careful survey of the recent discussions of the economic and political problems raised by economic planning. Though similar in purpose to Mr. T. J. B. Hoff's unfortunately still untranslated *Oekonomisk Kalkulasjon I Socialistiske Samfund* (Oslo, 1938), it does not give quite as lucid or as comprehensive a survey as the latter work, apparently because of the linguistic limitations of the author which probably also prevented him from utilising Mr. Hoff's book. And since the present work was submitted as a thesis in June, 1940, it carries the story hardly beyond the point at which Mr. Hoff left it and does not take into account some of the more recent contributions. It cannot be said that it makes any original contribution of importance. But for those who want an English summary of the present state of the debate it will prove a welcome and reliable guide.

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[1] Italics reviewer's.

*Does Money Matter?* By OSCAR R. HOBSON. P. S. King & Staples, Ltd. 1942. 118 pp. 2s. 6d.

This collection of some fifty of the short notes contributed to a daily paper by one of the most distinguished among contemporary financial journalists and selected from the much larger number which appeared in the course of a year of war, deserves the attention of the economist in more than one respect. As instances how difficult problems can be explained with the greatest economy of words in simple and clear language these notes have probably few equals. The combination of a healthy realism with a background of sound economics and a judicious temper which they show is at least uncommon. There will be few economists who cannot learn something of substance from these brief paragraphs. There are probably none who cannot learn a great deal from this little book about how to make an economic argument intelligible to the layman; and as a collection of model essays to be placed in the hands of the students, it should be invaluable to the teacher.

*On Social Freedom.* By JOHN STUART MILL. Reprinted from the *Oxford and Cambridge Review*, June, 1907. With an Introduction by Dorothy Fosdick. New York: Columbia University Press. 1941. 69 pp.

*The Spirit of the Age.* By JOHN STUART MILL. Introductory Essay by F. A. v. HAYEK. The University of Chicago Press. 1942. xxiii + 94 pp. $1.50.

These two reprints of little known essays by John Stuart Mill, issued by American University Presses, testify of a remarkable revival of interest in Mill in the United States. The essay on *The Spirit of the Age* was written by Mill at the age of 25 at the beginning of his career as an independent thinker when he had sharply reacted against his education. The essay *On Social Freedom* was probably written shortly before his death in 1873. Neither had been reprinted since it was first published in the pages of a not very accessible periodical, the first in the *Examiner* for 1831, the second posthumously in the *Oxford and Cambridge Review* for 1907. They are both now edited with fairly long introductions, Miss Fosdick's to the later essay largely doctrinal in character, Professor Hayek's on "John Stuart Mill at the Age of Twenty-Five," almost entirely biographical.

*Economic Relativity*. By GEORGE O'BRIEN. Statistical and Social Inquiry Society of Ireland. Dublin. 1942. 32 pp. 2s. net.

In this "Opening Address", delivered to the Statistical and Social Inquiry Society of Ireland in its ninety-fifth year, Professor O'Brien moves over so much ground that the future historian of our time may well use it as a convenient cross-section of the problems which mainly occupied economists and social philosophers during the war years. It gives evidence of great learning, a remarkable catholicity of taste, and a strong determination to find some middle or compromise position where there seem to be irreconcilable divergencies of view. As the author says in the concluding sentence, his emphasis is "on the limitations rather than on the positive achievements of economic science . . . because the success of these achievements depends on their being confined to the proper scope within which the economist can claim to speak with authority."

*The Network of World Trade*. A Companion Volume to "Europe's Trade". By the Economic Intelligence Service of the League of Nations. Geneva. 1942. London: Allen & Unwin, Ltd. 172 pp. 10s. net.

This is a most valuable and instructive addition to the economic publications of the League of Nations which one would wish to see much more widely read than is generally true of the wartime publications of that institution. A clearer and more comprehensive picture of the economic interdependence of the whole world has probably never been given before. It is strongly to be recommended to all those who wish to form a clear idea of the effects which any regulation of international trade on a "bilateral" or "regional" principle would have. The excellence of the arrangement of the material and the brevity and lucidity of the explanatory text make it equally useful as a work of reference and in University teaching. It deserves to be widely used as a textbook and to become the constant companion of those responsible for the shaping of the commercial policy of the future.

*A Dimensional National Economy*. By DAVID ATKINS. P. S. King & Staples, Ltd. 1942. xiv + 228 pp. 17s. 6d.

Since this book is published by a publishing firm well-known for the serious contributions to the literature of the social sciences and sold at a price of 17s. 6d., exorbitant for any book of its size, the readers of ECONOMICA might like to see the following specimen of its reasoning. The passage quoted purports to be a refutation of Marshall's "law of satiable wants" and occurs on p. 50 of the book:

"The writer has produced over 5,000,000 heads of celery on a single farm in a single season, and even though he regards celery as a particularly obnoxious vegetable, owing to its capacity for interfering with really valuable conversation at a dinner table, he can testify that (to him) the economic utility of the last heads of celery produced and shipped just prior to Christmas was not only greater than that of the first heads, but greater than the average. The numerator of this value was a single irrigated farm in California; the denominator was Chicago and all points East, *multiplied by* 90 *days*.

"If we express economic value as $\frac{\text{Quantity} \times \text{Quality}}{\text{Desire} \times \text{Time} \times \text{Area}}$ where has marginal utility gone? Mathematically, we are further from marginal utility, or the threat of satiety, than is a flat-iron released from a balloon, since in an unobstructed system an economic 'weight', or 'worth', is capable of bouncing back to the exact point from which it started, under the law of inertia, as it applies to the round trip. If this weight or worth overcomes a resistance in its path there may *appear* to be a loss, as in the case of rent, but, in economics, the value is transferred, *exactly*, to the resistance overcome".

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# Comparisons of Real Income

By A. C. Pigou

We suppose that in two different periods the same aggregate volume of productive resources, constituted in the same manner, is at work producing real income. The two real incomes will, in general, be made up differently, embracing the several sorts of commodities that are being produced in different proportions. The differences between them may be caused in any of three ways: (*i*) through a shift in productive technique alone, (*ii*) through a shift in demand conditions alone, and (*iii*) through shifts both in productive technique and in demand conditions occurring at the same time. I shall consider these three cases in turn.

## I

Three types of change in productive technique should be distinguished. First there may be a change that enables *certain* quantities of one particular commodity to be produced at less cost in resources than before. Obviously there will be one quantity, namely a *nil* quantity, of the commodity that no technical change can cause to be produced more cheaply or more dearly. But, apart from this limiting case, it may well happen that some technical devices are only applicable to some, e.g. very large, scales of output, for which mass production methods are suitable. In that case a single technical change of this type enables the yield of our fixed stock of productive resources to be increased in respect of all patterns of real income that contain any quantity of the commodity which it has become possible to produce more cheaply; but not in respect of other patterns.

Secondly, there may be a complex change of technique containing several elements and affecting several commodities, but all in the same sense. This type of change enables the yield of our resources to be increased in respect of all patterns of real income containing, for *any* commodity, a quantity such that the improvement—and correspondingly, of course, with a disimprovement—is operative.

Thirdly, there may be a complex change of technique containing several elements, some of which are favourable to productivity, others unfavourable. In consequence of such a change the yield of our resources will be increased in respect of some patterns of real income and decreased in respect of others. Evidently the yield is especially likely to be increased for patterns of real income containing large values (in terms of productive resources) of commodities whose production the change has affected favourably in a large measure; and conversely.

It is not difficult to see that it is *only* the one or other of these ways—only through some shift of technique—that a given volume of productive resources can be enabled to give an altered yield in respect of any assigned pattern of real income.

When any of the kinds of change that we have seen to be possible comes into play the effect on the size of a real income of any assigned pattern must always be unambiguous; the real income must be enlarged, left unaltered or diminished in some specifiable degree.

This is not to say, however, that after a change of technique the real income that rules then must always be unambiguously greater than, equal to or less than the real income of different pattern that ruled before the change. This need not be so. Thus, even if the source of difference is an improvement—analogous reasoning applies to a worsening—of technique affecting one commodity only, and that commodity is not a rival to anything else, should the demand for it have an elasticity greater than unity, the effect of the improvement will be to draw resources towards it from elsewhere, so that the output of some other commodity is affected adversely; and, when the commodity is a rival to something else, it is practically certain that the output of the other thing will be affected adversely. *A fortiori*, this may happen when technical changes occur that combine some improvements and some disimprovements. Clearly, however, when the output of some things is increased and of others diminished, it is not possible to say whether real income as a whole, conceived as a purely physical entity, has been increased or diminished. The real income of Period II is neither unambiguously larger nor unambiguously smaller than that of Period I.

Another relation is, however, possible between the real incomes of two periods besides that of greater or less. One may be unambiguously *preferred* to the other, even though it is not unambiguously either larger or smaller. By being preferred I do not imply anything about mental states—a field which, when a number of different people are involved, is best avoided; I mean simply that the one real income is, or would be, chosen rather than the other when the two are presented as alternatives. If real income *A* is larger than real income *B*, it must obviously be preferred to it in *any* given set of demand conditions; but the converse does not hold. A larger real income is a particular species of the more extended genus, preferred real incomes. We have to enquire how the real incomes produced before and after changes of technique stand in regard to this more general relation.

The first step is to demonstrate a proposition which is, *prima facie*, somewhat surprising. This is that, *given the state of demand*, when a shift of technique enables more of the real income of the pre-change period—call this Period I—to be produced, the pattern of real income established in Period II must be such that the shift also enables more real income of that pattern to be produced. The proof is as follows. Let it be that before the shift of technique { $x$ of one thing plus $y$ of another } are being produced; after the shift { $x'$ of one thing plus $y'$ of another }. After the shift $m$ { $x$ of one thing plus $y$ of another } could have been produced; where $m$ is $> 1$, thus indicating that our resources are enabled by the shift to produce more real income of the pattern { $x$ of one thing plus $y$ of another }. Therefore { $x'$ plus $y'$ } is preferred to { $m$ ($x$ plus $y$) }. Before the shift { $x$ plus $y$ } was being produced, whereas { $n$ ($x'$ plus $y'$) } could have been produced. Therefore { $x$ plus $y$ } is preferred to { $n$ ($x'$ plus $y'$) }. But since $m > 1$, { $m$ ($x$ plus $y$) } is preferred to { $x$ plus $y$ }. Therefore, *a fortiori*, { $m$ ($x$ plus $y$) } is preferred to { $n$ ($x'$ plus $y'$) }. Therefore, again *a fortiori*, { $x'$ plus $y'$ } is preferred to $n$ { $x'$ plus $y'$ }. Therefore, $n < 1$. That is to say, our resources are enabled by the shift to produce more real income of the pattern { $x'$ of one thing plus $y'$ of another thing }. More generally, in the conditions we are supposing it necessarily happens that a shift of technique affecting in a given sense the real income of the pre-shift period must lead to the establishment in the post-shift period of a pattern of real income which is affected by that shift in the same sense.

On this foundation we proceed as follows. Suppose first that a shift of technique takes place increasing productivity in respect of the pre-shift pattern of real income. After the shift an unambiguously larger real income of that pattern could be made. The new actual real income after the shift must, therefore, be preferred to a post-shift real income larger than the pre-shift one. *A fortiori* it must be preferred to the pre-shift real income itself. But now change the point of view. Apart from the proposition established in the last paragraph, it might be that the same shift in technique which made a real income of pre-shift pattern larger at the same time made one of post-shift pattern smaller. If this were so, actual pre-shift real income would be preferred to actual post-shift real income. We should thus be involved in a contradiction. It has been shown, however, that when a change in technique makes either a real income of the pre-shift or one of the post-shift pattern larger, it must make both larger; and conversely. Therefore, situations in which the contradiction would emerge cannot exist. A technical change that enables either the real income of pre-shift pattern or that of post-shift pattern to be enlarged enables both to be enlarged, and, therefore, without any ambiguity, entails the post-shift real income being preferred to the pre-shift one; and conversely. It may happen, of course, we may add in passing, that the unambiguously preferred real income is also unambiguously the larger. But this need not and frequently will not happen.

## II

Let us now abandon the assumption that the state of demand is held constant between the periods we wish to compare, and suppose that this state alters, while no shift of technique of any kind takes place. The real income of the second period must then contain more of some items and less of others than the real income of the first period. Thus it is neither unambiguously greater nor unambiguously less than that real income of the first period. From the standpoint of the demands of the first period the real income of the first, from that of the demands of the second period the real income of the second, is obviously preferred. Thus, when demand conditions are altered, we cannot say, as we might, perhaps, have wished, that unaltered productivity of resources entails either the real income of the second period being equal in point of size or being equally preferred with that of the first period; but we can say that neither real income is unambiguously greater than and neither is unambiguously preferred to the other.

## III

Suppose, thirdly, that a shift of technique occurs at the same time as an alteration in demands. In these conditions we have no ground for holding, as we had in the case of constant demand conditions, that the productivity of resources must be affected in the same sense in respect of the pre-shift and the post-shift real income patterns. *If* it is affected in the same sense in respect of both patterns, the real income of the period for which resources are thus doubly more productive is preferred to the real income of the other from the standpoint of the demands of both periods. It is thus unambiguously preferred to the other. It *may* happen here, just as in conditions of constant demands, that the preferred real income is also unambiguously the larger real income. If the shift of technique affects the productivity of our resources favourably in respect of the income pattern of Period I and unfavourably in respect of that of Period II, it is easy to show, on the lines of the preceding analysis, that the real income of Period II is preferred to that of Period I from the standpoint of the demands of Period II; while the real income of Period I is preferred from the standpoint of those of Period I. Thus we cannot say that either real income is unambiguously preferred to the other.

## IV

It remains to enquire what light this enquiry can throw on the significance of indices of real income. The essential characteristic of such indices is that they set against the real income of every period some definite number. As between any two periods the number attached to one is necessarily larger than, equal to or less than that attached to the other. As we have seen, even between periods for which the state of demands is constant, it will frequently happen that the real income of neither is unambiguously larger or smaller than that of the other. Therefore, it is in the nature of things impossible for any system of indices to provide numbers such that a larger number usually, or even often, indicates a larger real income. Where, however, demands are constant, all real incomes resulting from a given volume of productive resources are related unambiguously in respect of preferredness. Hence it is not in the nature of things impossible to construct an index such that a larger number shall always indicate a *preferred* real income. But in actual life states of demand are, of course, not constant; on the contrary, they are liable to frequent change. In these conditions, as we have seen, it will often happen that the real incomes of different years are related to one another ambiguously, not merely in respect of size but also in respect of preferredness. It is then in the nature of things impossible to construct an index such that a larger number always, or even usually, indicates a preferred real income. The relation between the numbers suggests that between any two real incomes one must always be in some sense superior to, equivalent to, or inferior to the other. Unless, however, there is some relevant kind of superiority other than preferredness or larger size, this suggestion is false[1]; no index number can be constructed which does not on many occasions profess to tell us something when, in fact, there is nothing to tell.

### NOTE

Given constancy of demands it is not difficult—in theory—to devise an index such that a higher number will always correspond to a preferred real income. One index which satisfies this condition is fashioned thus. For simplicity of exposition—no point of principle is involved—suppose that money aggregate income, as well as the volume of resources at work, is the same in the different periods that are to be compared. Real income must then be supposed to vary directly with the purchasing power of a £; so that the reciprocals of a general price index constitute indices of real income. Let the real income of Period I be $x_1$, $y_1$, . . . . with average cost of production in the conditions of Period I $a_1$, $b_1$, . . . .; in the conditions of Period II $a_2$, $b_2$ . . . . . : and let the real income of Period II be $x_2$, $y_2$ . . . . with average cost of production in the conditions of Period I $\alpha_1$, $\beta_1$ . . . . , in the conditions of Period II $\alpha_2$, $\beta_2$ . . . If we put 100 for the income index of Period I, the numbers for Period II in respect of the two patterns of real income are

$$100\ \frac{x_1 a_1 + y_1 b_1 + \dots}{x_1 a_2 + y_1 b_2 + \dots} \text{ and } 100\ \frac{x_2 \alpha_1 + y_2 \beta_1 + \dots}{x_2 \alpha_2 + y_2 \beta_2 + \dots}$$

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[1] A good illustration of false suggestion is provided when tastes are shifted from diminishing-return commodities to increasing-return commodities, while technique undergoes no shift. This would entail a reduction in price of both sorts of commodities, so that practically any kind of index number would indicate an improvement in real income. In fact, in these conditions the later and the earlier real incomes are ambiguously related in respect both of size and preferredness.

For brevity write these expressions $\frac{P_1}{P_2}$ and $\frac{\pi_1}{\pi_2}$

It can be shown on the lines of the foregoing analysis—*provided demands are unchanged*—that the two real incomes must be so related that $\frac{P_1}{P_2}$ and $\frac{\pi_1}{\pi_2}$ are either both $> 1$ or both $< 1$; and further that, according as any of $\frac{P_1}{P_2}$, $\frac{\pi_1}{\pi_2}$ and $\sqrt{\frac{P_1}{P_2} \cdot \frac{\pi_1}{\pi_2}} >$ or $< 1$, the real income of Period II is unambiguously preferred to the real income of Period I or the reverse. Thus an index of any of these types satisfies the required conditions. Unfortunately, however, such an index cannot be constructed in practice because there are no means of discovering the values of $a_2$, $b_2$ . . . and $\alpha_1$, $\beta_1$ . . .

Quantity indices based on Fisher's "ideal" price indices put the ascertainable values $\alpha_2$, $\beta_2$ . . . in place of $a_2$, $b_2$ . . . and the ascertainable values $a_1$, $b_1$ . . . in place of $\alpha_1$, $\beta_1$ . . . In the special case of universal constant returns the resultant numbers are identical with those set out above. Apart from this case, however, they are not identical. They are likely to differ from them more largely, the more numerous are the commodities the conditions of whose production diverge from constant returns, and the wider the divergencies are.

# The Expectational Dynamics of the Individual

### By G. L. Shackle

## I

That new part of economic theory which seeks to explain the level of employment and the rate of flow of output as a whole had its historical origin in the theory of money. Thus it was held that the quantity of money existing at a given moment determined in some way the general level of prices, and that this level, or its changes, determined whether enterprisers would make profits or losses, and thus whether they would extend or reduce the scale on which they engaged in the making and selling of goods, and the quantity of employment they offered. It was possible to develop a self-consistent theory on these lines without any reference whatever to the process of thought, the motives, and the temperament of the *individual enterpriser*. He was assumed to react in an almost mechanical way to a change in *current* prices or profits. Thus while in the theory of value the individual was king, and his preferences and reactions were the fundamental theme, in the theory of money he found no place at all. The theory of money has developed into and been absorbed by what is now sometimes known as macro-dynamic economics, and this term itself implies that we are concerned with phenomena " in the large " and can neglect the individual. Yet the most modern forms of economic dynamics admit that it is, after all, the decisions of *individuals* which determine what will happen. Professor Hayek, for example, has defined equilibrium as that state of affairs where the plans of *individuals* are mutually compatible. And the need to study what goes on in the mind of the individual is also tacitly admitted by modern dynamic theories in the importance they assign to *expectations*.

Judgment of what is implied for the future by what we know of the present situation, summarising past history within itself, and above all, the choice of what *assumptions* to make, what products of almost pure artistic imagination to introduce, in order to supply the deficiencies of this knowledge and complete in some rough sense the data for logical inference as to the future course of events, will vary at least as much between different individuals as their preferences. Yet there has been no open recognition so far in economics of the need to study the psychology of expectation as a process of the individual mind. It would be hard to exaggerate the difficulty of the field thus

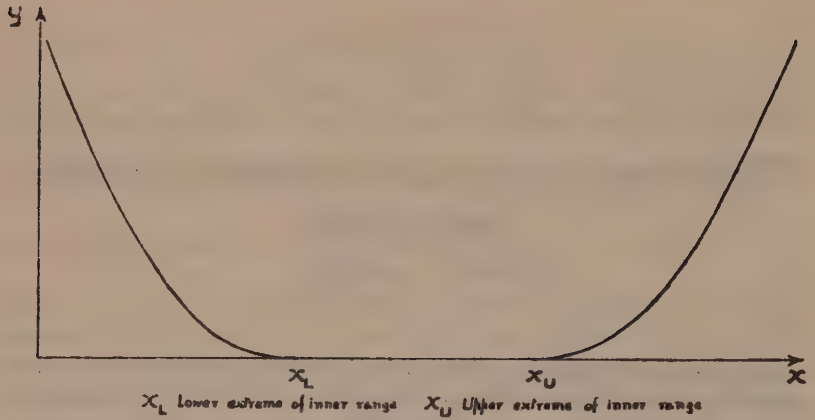

FIGURE 1

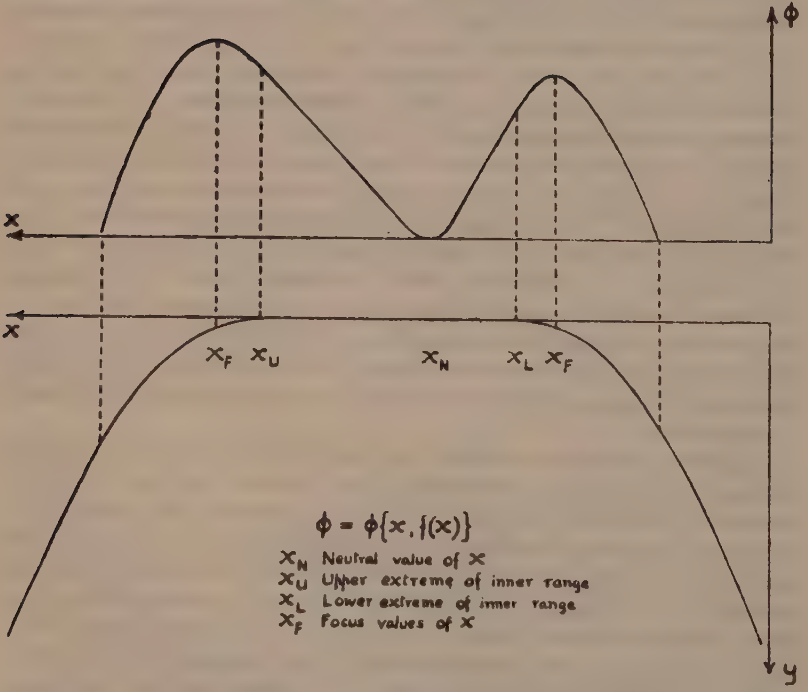

FIGURE 2

opened up. The central problem of all is the nature of the mental process by which a man considers, first, the entire set of all rival courses of action open to himself, and second, the entire set of all the different paths which the future course of events could follow, and decides which members, that is, what sub-set, of the second set correspond to each member of the first set. In what follows,[1] I have done no more than indicate some of the limitations of our power to establish this correspondence, and traced some consequences which follow from this limitation ; and have then tried to indicate, in a very tentative way, what seems to me a possible line of attack on the core-problem of this subject, the mode of formation of expectations. Many of the suggestions advanced in this article are, I believe, possible constituents of a *general* theory of expectation, and are not confined to the economic field by means of which they are here illustrated.

## II

Between a feeling of certainty that a given event will happen (or some particular answer to a given question turn out to be the truth), and a feeling of certainty that it will not, there seems to be a continuous range of different levels at which our degree of belief can stand. By what operation can a person compare his own respective degrees of belief in two different outcomes of some course of action or two different answers to a question ? I think he can do this by taking each of these outcomes or answers in turn and asking himself what intensity of shock or surprise he would feel if, without there having been any change in the knowledge available to him on which he based his belief in it, he were to learn that this belief is mistaken. That is to say, the operation by which he measures his degree of belief, corresponding for example to the application of a column of mercury in measuring temperature, is the act of imagining himself to learn that this belief is false. The measure so obtained is what we may call the *potential surprise* associated with the falsity of the particular answer or the non-occurrence of the particular hypothetical outcome. In some circumstances it will be appropriate to ask ourselves what degree of potential surprise we attach to the fulfilment rather than the non-fulfilment of a given hypothesis. For suppose we are not concerned simply with the question whether one single particular hypothesis is true or untrue, but rather with the question *which* out of a set of rival hypotheses is the true one. In this case there may be a sub-set of several hypotheses, each member of which is superior, as regards our degree of belief in it, to any hypothesis outside the sub-set, but of which no one member is superior to any other member. In this case it will be impossible for us to attach any non-zero degree of potential surprise to the non-fulfilment of any particular member of the sub-set: for to do so would *ipso facto* mark it off as claiming a higher degree of our belief than the other members. The most we can do is to attach *nil* potential surprise to the *fulfilment* of any member of the sub-set. But we *can* attach some positive degree of potential surprise to every hypothesis *outside* the sub-set, and by doing so we shall express its inferiority in the matter of the degree of our belief in it, to every member of the sub-set. And further, we can attach different degrees of potential surprise to different hypotheses outside the sub-set, thus assigning to them positions on a scale of belief. If the sub-set has only one single member, and if for *every conceivable* non-member the degree of potential surprise attached to the fulfilment of this non-member is indefinitely high, this will mean that we feel completely certain that the one member will turn out to be the truth.

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[1] The exposition in Sections II and III of this article will be found to follow closely the lines adopted in my article " A Theory of Investment Decisions " (*Oxford Economic Papers*, No. 6), though the argument is more explicitly stated on some important points. Sections IV to VIII of the present article contain an entirely fresh formulation of two of the main themes of the former article, and in addition open a new theme by considering the logical structure of enjoyment by anticipation and a consequence which flows from it. Section IX opens a discussion of the problem of how expectations are generated.

By the *outcome* of a given course of action we shall mean the entire set of advantages and disadvantages which the adoption of this course, rather than some other which is being compared with it, will bring to the individual concerned. Experience teaches us that he will virtually never be able to specify for each of these courses a unique outcome which he can feel completely certain will turn out to be the true one. For any one course of action he will have in mind many different hypothetical outcomes, and out of these there will be a large number any one of which would cause him some degree of surprise if it were to turn out to be the actual one without there having been, in the meantime, any change in the knowledge on which he is now basing his expectations. The degree of this potential surprise will differ between different hypotheses. Some will seem to involve assumptions which conflict with elements in the present situation, and therefore to involve the further assumption that special factors will arise, of which there is no evidence at present, capable of cancelling these adverse elements. The potential surprise attached to these hypotheses may be extreme. Other hypotheses will seem potentially less surprising, and others less still, in descending steps which will lead ultimately to the other group of hypotheses, those, namely, for each of which the potential surprise is *nil*. Now it is clear that this " inner " group must comprise at least one hypothesis: but our knowledge of the present intentions and of the capacities of other people, and of what will be their reactions in the further future to each other's more immediately future acts, is so extremely slight and insecure that, in reality, the inner group will always consist of a large number of hypotheses whose mutually most dissimilar members will differ from each other very widely. This conclusion brings us to our central problem: How will an individual choose between different courses of action when the outcome of each cannot be known in advance, but is only represented in his mind by a whole set of differing hypotheses?

The real incentive for embarking on some given course of action, whose objective results will not develop and their character become known until some date in the future, is the *immediate* mental experience which the decision to embark on this course will give us, namely, the *enjoyment by anticipation* of a high level of success. I do not believe that the impossibility of feeling *certain* that a particular unique result will be attained by the contemplated course of action implies that the individual will not desire a unique focus for his imagination, that is to say, that he will not centre his hopes on *one particular* level of success. He will, it seems to me, desire a single clear-cut mental image concerning the outcome to provide the *content* of his hope, and this will need to be given, as it were, an extra dimension, by association with a sufficiently low degree of potential surprise, to render it a motive force behind his decision. There is a phrase of common speech which seems to throw much light on this matter (such phrases, moulded unconsciously by the actual modes of working of our minds, of many minds over many centuries, should surely be a safe and fruitful source of instruction): we speak of having " a *lively* expectation " of something. This must surely mean a *life-like*, *vivid and active* expectation, one which carries conviction by the realism and insistent presence, as it were " in the flesh ", of the imagined thing. A lively expectation, something which seems a living projection of the future into the present, is what is needed to induce us to take action now to attain the reality. This " liveliness ", this realism, cannot conceivably be possessed by a bundle of divergent hypotheses or conceptions: it must require uniqueness in the mental image on which we finally focus our attention, even if there is no such uniqueness in the contingencies we pass in review in our minds. The question that we have to answer, if this view be accepted. is, how will the particular level of success upon which he will settle his hopes be determined from amongst the whole range of hypothetical outcomes which in varying degree he entertains ?

The enjoyment by anticipation of a favourable result will not be experienced unalloyed, except in the rare case where the outcome is felt to be uniquely certain. In most cases there will be some positively hurtful outcomes which will be not less insistently present to our imagination than the favourable ones. Here again the individual will tend, I think, to concentrate his fears at some specific point, selecting *one particular* degree of misfortune to represent what he " stands to lose ".

Now the intensity of enjoyment of a given hypothetical outcome from imagining it in advance is a function of more than one variable, but it is surely a *decreasing* function of the degree of potential surprise attached to this outcome. It is also evidently an *increasing* function of the desirability of the outcome. In the same way the distress the individual will feel at the thought of a positively hurtful outcome will be a decreasing function of the potential surprise and an increasing function of the hurtfulness. Out of the whole assembly of hypotheses as to the outcome of any one course of action, those hypotheses the thought of which causes him the most enjoyment or the most distress will mainly focus his attention and influence him in assessing the attractiveness of this course of action in comparison with others. Thus, in general, there are two possible reasons for the individual to pay more attention to one of a pair of hypotheses than to the other: first, there may be a difference in the respective degrees of potential surprise he associates with them, and second, when both outcomes are advantageous one may be more advantageous than the other, and when both are hurtful one may be more hurtful than the other. But among all the outcomes for which his potential surprise is *nil*, there will be only one reason for him to concentrate his attention on some rather than others, namely, that some are more desirable or more undesirable. I suggest, therefore, that when he contemplates this inner group of outcomes each of which carries no potential surprise, the individual concentrates his attention on the *best* and the *worst* hypotheses in this range. Evidently he need not confine himself to considering only the inner group; outside it there may be outcomes even more desirable or hurtful than any of those inside it. But it will usually be true that outside the inner group, the greater the desirability or hurtfulness of a given outcome the higher the degree of potential surprise it will carry; and since, for some outcomes, the potential surprise is indefinitely great, amounting to absolute disbelief in the possibility of these outcomes, and since in such a case there can be no enjoyment of these outcomes by anticipation, there will be a point beyond which no outcome offers a sufficient extra advantage (or extra detriment) over the next most desirable (or hurtful) to compensate for the extra potential surprise which it carries. At such a point the total differential of the degree of enjoyment by anticipation, or distress by anticipation, will be zero, and the degree of enjoyment or distress a maximum. At these two points will be found that particular pair of hypotheses which will mainly capture the individual's attention and will represent for him the attractiveness of the particular course of action in question.

The set of hypotheses in an individual's mind regarding the outcome of a given course of action, in the sense of the net advantage or disadvantage that will result from it, can consist of all the possible values of a continuously variable quantity $x$. Let $y$ stand for the degree of potential surprise associated with $x$. Then we have seen that there will usually be a range of values of $x$ for all of which $y = 0$, this range being what we have called the "inner" group of hypotheses. We shall call it the "inner range". For all values of $x$ above the upper extreme of the inner range, $y$ will be an increasing function of $x$, and for all values of $x$ below the lower extreme of the inner range, $y$ will be a decreasing function of $x$. Thus the shape of the function $y = f(x)$ will usually be of the general type illustrated in Figure 1. It is reasonable to suppose that the segment of $y$ corresponding to the inner range of $x$ will merge smoothly into the decreasing and increasing segments of $y$, so that $\frac{dy}{dx}$ will be everywhere continuous. We shall call the function $y = f(x)$ the potential surprise function.

The intensity of enjoyment or distress caused by the thought of any hypothetical outcome will be a function, first, of the degree of success or advantage represented by this hypothesis, and second, of the degree of potential surprise associated with it. For this intensity of feeling, whether it be of enjoyment or distress, let us write $\phi$. Then $\phi$ will depend on $x$ in two ways, first directly, and second through its dependence on $y$, which itself depends on $x$. Thus we can write $\phi = \phi\{x, f(x)\}$. Now let us suppose that at some value *within* the inner range $x$ is such that the thought of it causes neither enjoyment nor distress; and for simplicity let us assume that this neutral value of $x$ happens to be $x = 0$. Over the interval of $x$ between zero and the upper extreme of the inner range $\frac{dy}{dx} = 0$ and therefore $\frac{\partial \phi}{\partial y}\frac{dy}{dx} = 0$, while $\frac{\partial \phi}{\partial x}$ is positive. Over this interval, therefore, $\phi$ will increase as $x$ increases. Beyond the upper extreme of the inner range, $\frac{dy}{dx}$ is positive, and therefore, since $\frac{\partial \phi}{\partial y}$ is everywhere negative, $\frac{\partial \phi}{\partial y}\frac{dy}{dx}$ is here negative, while $\frac{\partial \phi}{\partial x}$ is, of course, still positive. Now when $y$ approaches a value amounting to a feeling of complete certainty that the hypothesis will not be realised, $\phi$ will be reduced to zero whatever the value of $x$. But clearly in most concrete cases there are finite values of $x$ for which $y$ will be indefinitely great. There will therefore be some value of $x$ at which $\frac{\partial \phi}{\partial x} = -\frac{\partial \phi}{\partial y}\frac{dy}{dx}$ and at this point $\phi$ will be a maximum. Turning to the negative values of $x$ let us put $z = -x$, so that increasing values of $z$ stand for increasing degrees of hurtfulness or disadvantage in the outcome. Then an argument similar to the one above shows that there will be a point where $\frac{\partial \phi}{\partial z} = -\frac{\partial \phi}{\partial y}\frac{dy}{dz}$ and here $\phi$ will again have a maximum value. These two values of $x$, we suggest, where $\phi$ is a maximum, will attract to themselves the greater part of the individual's attention, and we shall call them the *focus-values* of $x$. His basis for comparing the attractiveness of the course of action in question with that of another course will consist in these two values.

The function $\phi = \phi\{x, f(x)\}$ will resemble in shape the one shown in Figure 2. This figure ought, of course, to be a three-dimensional model with the $x$, $y$ plane and the $\phi$, $x$ plane perpendicular to each other. In Figure 2, however, the $x$, $y$ plane is considered to have been rotated about the $x$ axis through one right angle, so that both planes lie flat on the page. Thus in the upper half of the diagram we see the projection of the $\phi$ curve on the $\phi$, $x$ plane, while in the lower half we see the $y$ curve placed so that each point on it lies in a position on the page exactly beneath that point on the $\phi$ curve which corresponds to it. In the true three-dimensional model, $\phi = \phi\ \{x, f(x)\}$ would, of course, be a twisted curve whose projection on the $x$, $y$ plane would coincide exactly with the $y$-curve.

## III

By the earnings of a piece of equipment in any year we mean the difference between the receipts in that year from selling its product and the outlay in that year for the materials and the services needed to operate it. If the assumed earnings of each future year of its useful life are discounted to the present at the rate of interest now obtainable on a riskless loan repayable at the same date, and the results for all these years added together, we get a " present value " for the plant. The difference between the present value of a proposed new plant and what this would cost to construct is what we mean by the gain or loss from constructing such a plant. This gain or loss is a specially clear-cut example of an outcome to which the conception of focus-values described above is appropriate. Even the construction-cost, though it refers to proposed operations in the immediate future, cannot always be exactly known in advance. The *value* of a new plant to an owner who intends to retain possession of it during the whole of its working life clearly depends on the circumstances of a long series of years stretching into the future. If instead he hopes to sell the plant, once he has brought it to concert pitch as a going concern, then its value to him depends on what he thinks will be the opinion concerning those future years held by the most sanguine potential purchaser ; or on what that purchaser himself thinks will be the opinion of some subsequent purchaser ; and so on indefinitely.[1] Clearly in either case, whether the potential constructor intends to hold or to sell the plant, the present value to be assigned to it, while it still exists only in his mind, is open to extremely divergent conjectures. Thus the outcome of constructing the plant will present itself as a wide range of hypotheses varying from a gain of several times the amount laid out on construction to the loss of the whole of it, and a considerable inner range of these hypotheses will carry *nil* potential surprise. The choice which faces the holder of a large sum of cash, between putting into execution one or other of a large number of projects for constructing equipment of widely differing characters, or retaining the cash or lending it at fixed interest, will be resolved according to our theory by comparing, in the manner suggested in the next paragraph, the pairs of focus-values assigned to the different courses of action.

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[1] The dependence, e.g., of Stock Exchange values on what people think other people are going to think is going to happen, and so forth, was first pointed out by Lord Keynes in *The General Theory of Employment, Interest and Money*, Chapter 12.

Let us use the term " blueprint " to mean a project, existing only in a person's mind or on paper, for constructing a block of equipment of a specific character. One of the focus-values which the individual assigns to the outcome of each blueprint will usually represent a gain and the other a loss, and we shall call these the " focus gain " and the " focus loss " of the blueprint. If we write $v$ for the value and $c$ for the construction cost of a plant, and put $x = v - c$, then positive values of $x$ will represent a gain and negative values a loss, and we could speak of a *numerical* increase of the loss $c - v$ as a decrease of $x$. But for simplicity of statement we shall say that the loss increases when we mean that it increases *numerically*, passing to an algebraically smaller value of $x$. We shall sometimes speak of the focus-values of $x$ as the " focus-outcomes " of a given course of action. Suppose that for a blueprint $A$ the focus gain is $g_A$ times the construction-cost and the focus loss a proportion $h_A$ of the construction cost, while for a blueprint $B$ the ratios are $g_B$ and $h_B$. For each ratio $\frac{h_B}{h_A}$ there will be some critical ratio $\frac{g_B}{g_A}$ which would make blueprint $B$ neither more nor less attractive than blueprint $A$. If $\frac{g_B}{g_A}$ exceeds this critical level, the investor will decide, so far as the choice between $A$ and $B$ is concerned, in favour of $B$. All other pairs of uses for his cash can be compared in the same way and his course thus decided on.

## IV

A blueprint has amongst its attributes not only the nature of its product and its own technical design and its location, but also the specific period of future time in which it is intended to be operated. An investor who has in mind at a particular date a given type of plant, will assign to it different pairs of focus-outcomes according as he considers constructing it now or at various distances in future time. For not only will he expect the circumstances of two different specific periods to differ, but the more distant the period considered, the less light his present knowledge will seem to throw on it, and the wider will be the range of values of each important variable[1] in the period which he must regard as non-surprising. Thus if we designate different physical types of plant by letters $A$, $B$, etc., we must also attach to each letter a subscript indicating the particular fixed date[2] at which we are supposing that the plant will be completed and begin its operating life.

Let each of the symbols $A_{[1]}$, $A_{[2]}$, -- $A_{[r]}$ -- represent the idea of constructing a plant of given technical design on some fixed, specified date, denoted by the subscript, the design of the plant being the same for all the dates. When the present moment is located at date [1] the investor will have in mind for each of these blueprints its own schedule assigning to each hypothetical outcome of this blueprint its particular degree of potential surprise. That is to say, at date [1] he will have in mind for any blueprint $A_{[r]}$ a unique specific form of the potential surprise function, such functions differing from each other in general for blueprints of different construction dates. But this form which he can specify *now* is not the only form of the potential surprise function for $A_{[r]}$ which he must take into account. He can foresee that when the present moment will have arrived at date [2], and again at date [3], and so on, up to date [r] itself, he will construct a fresh potential surprise function for $A_{[r]}$ which, since at each of those dates he will have in mind knowledge and ideas which will have come to him in the interval, can differ from the form he assigns now. He cannot, of course, specify exactly *now* the form of function that he will assign to $A_{[r]}$ *at date* [*r*]; but we shall see that he can sometimes form some judgments about it which are as important, for deciding on his present action, as the form which he assigns now. For convenient reference we must have a notation distinguishing from each other the ideas, present in his mind at date [1], of constructing potential surprise functions for $A_{[r]}$ at different future dates. For this purpose, let us attach to the letter $y$, standing for some form of potential surprise function, two subscripts, of which the first will specify the *viewpoint* or moment at which the investor is actually doing his thinking, and the second will specify a date, which may be the same as the viewpoint or may be still in the future, at which he will be assigning a particular form to $y(x)$. Thus $y_{[1][3]}, A_{[3]}$ is the identity-disc, as it were, which the investor attaches, at date [1], to that particular form of the potential surprise function which he will assign to $A_{[3]}$ at date [3]. By its nature $y_{[1][3]}, A_{[3]}$, if it is other than identical with $y_{[1][1]}, A_{[3]}$, clearly cannot be a unique form, but must consist in two or more hypotheses. When the investor's viewpoint or present moment arrives at date[3] then $y_{[3][3]}, A_{[3]}$ will come into existence and will be a unique form, and any differences between this form and $y_{[1][1]}, A_{[3]}$ will have been due to fresh news or inferences which came into the investor's mind between dates [1] and [3]. Thus so far as he can make, at date [1], any judgments as to what questions will be answered by the fresh news or knowledge which will come to him before date [3], he can give to the symbol[1] $y_{[1][3]}, A_{[3]}$ a content consisting of a number of hypotheses, each of which will be a different specific form of potential surprise function, and will correspond to one particular set of answers to the questions; and *to each of these hypotheses he can attach its*

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[1] Such as prices, total value of backlogs of orders, size of inventories in various hands, rates of taxation, relative numerical strength of parties in Congress or Parliament, etc., etc.

[2] i.e., specified date such as January 1st, 1944.

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[1] For clearness of presentation, we will continue throughout this section to use an actual numeral [3] to represent a future date. It will be understood that the use of the particular numeral [3] for this purpose has no significance: any other numeral would have done. In later sections we shall represent future dates by [n].

*particular degree of potential surprise.* If any of these hypotheses assigns to $y(x)$ a *lower* value than is assigned to it, for the same value of $x$, by $y_{[1][1]}$, $A_{[3]}$, then the investor will attach to this hypothesis an indefinitely large potential surprise: for a value of $x$ which he thinks, at date [1], will be potentially surprising to him at date [3] cannot be less potentially surprising to him now: any other state of mind involves him in a logical contradiction. But there is no logical hindrance to his attaching *nil* potential surprise to any of the hypotheses constituting $y_{[1][3]}$, $A_{[3]}$ which assigns to $y(x)$ a higher value than is assigned to the latter, for the same value of $x$, by $y_{[1][1]}$, $A_{[3]}$. Thus it is in the nature of focus-outcomes that the investor will attach indefinitely high potential surprise to the idea that the focus-gain or the focus-loss of a blueprint of distant construction date might increase as the present moment advances towards that date. But he can, so far as logical consistency is concerned, and in some circumstances will, attach *nil* potential surprise to the idea that either of them may decrease.

The possibility that the focus-gain or the focus-loss of a blueprint of distant construction date may get smaller as that date approaches will usually arise in the individual's mind in a special form: he may have in mind one or more particular questions, such as what will be the result of some election, the provisions of some new statute, the character of a Budget, or the outcome of a mineralogical survey, whose answers if they are of one kind will curtail the inner range at its upper end or steepen the slope of the increasing segment, or both, and will leave the rest of the curve unchanged; and if they are of an opposite kind, will curtail the inner range at its lower end or steepen the negative slope of the decreasing segment, or both, and leave the rest of the curve unchanged. We shall refer to a change of form involving only some part of the curve above the lower extreme of the inner range, whether this change involves a curtailment of the inner range or a steepening of the increasing segment, or both, as a shift of the *upper branch* of the curve; and we define correspondingly, *mutatis mutandis*, a shift of the lower branch.

If the investor attaches some positive degree of potential surprise to the two equivalent ideas:

(1) that the questions will not be answered before date [3];

(2) that $y_{[1][3]}$, $A_{[3]}$ can be considered unique and identical with $y_{[1][1]}$, $A_{[3]}$;

then he cannot at the same time discriminate, by attaching some positive potential surprise to one of them and *nil* to the other, between a shift of the upper branch of the curve, implying a decrease of the focus gain, and a shift of the lower branch, implying a decrease of the focus loss. For if he did thus discriminate, this would mean that *both* the non-answering of the questions, *and* the shift of one of the two branches, would carry some positive potential surprise; and since these are the only two alternatives to a shift of the *other* branch of the curve, *the non-occurrence of this latter shift* would carry some positive potential surprise; but in this case it would no longer be something which might occur in the future, but would be already embodied in the form he assigns *now* to $y(x)$, namely $y_{[1]\,[1]}$, $A_{[3]}$. On the other hand, if he attaches *nil* potential surprise to the *non*-answering of the questions, he can then attach some positive potential surprise to a shift of one branch of the curve and *nil* to a shift of the other.

The special questions may sometimes, of course, have not merely two but any number of different possible answers: for example, the different answers can consist in different values of a continuous variable. In this case, some of the possible answers may imply an inward shift of *both* branches of the curve. But here again, provided that the investor attaches some positive degree of potential surprise to the non-answering of the questions on or before the future date he has in mind, then there must be amongst all the others two sets of answers, both carrying *nil* potential surprise, of which one set implies no shift of the upper branch of the curve specified by $y_{[1]\,[1]}$, $A_{[3]}$, and the other implies no shift of the lower branch.

The main conclusion to which this section has led us is the following: In regard to some particular blueprint, which can be either a blueprint for deferred construction or one for immediate construction, the investor can sometimes feel that he will be very surprised if *neither* the focus-loss *nor* the focus-gain decreases at some specific future date. In the following two sections we shall see two ways in which this feeling may induce him to refrain from immediate construction of a blueprint which he would otherwise find attractive. If for either reason he does so refrain, the aggregate national investment-flow will be lower, over some period of the immediate future, than it would otherwise have been.

## V

Let us suppose that the investor has in mind a blueprint for immediate construction $A_{[1]}$, which he would embark on, rather than retain his cash or lend it at fixed interest, if he had no other blueprint in mind. And let us suppose that he has in mind a blueprint for deferred construction $B_{[n]}$ to which $y_{[1]\,[1]}$, $B_{[n]}$ assigns an extremely high focus gain, but also assigns a focus loss so large that, if the blueprint's attractiveness depended on this pair of focus-outcomes alone, the investor would dismiss it from consideration. And further, let us suppose that $y_{[1]\,[n]}$, $B_{[n]}$ comprises a number of different hypotheses as to the form of the potential surprise function which the investor will assign to $B_{[n]}$ when date [n] is reached, and that amongst these there is one, carrying *nil* potential surprise, which would imply the same focus gain as $y_{[1]\,[1]}$, $B_{[n]}$, but a focus loss so small as to render $B_{[n]}$, in view of its high focus gain, an extremely attractive investment-opportunity. If the opportunity which would thus be created were actually and immediately open to him, the investor would construct it in preference to $A_{[1]}$. But the creation of this opportunity is in fact only a contingency, and both the occurrence *and the non-occurrence* of this contingency carry *nil* potential surprise. Accordingly, he must consider what alternative investment-opportunity would be open to him at date [n] should the focus-loss of $B_{[n]}$ fail to decrease. If this alternative is not less attractive than $A_{[1]}$, then a decision to retain his cash until date [n], in order to be assured of the power to venture on $B_{[n]}$ should he then wish to, will imply at worst a postponement and not the losing altogether of an investment-opportunity of the quality of $A_{[1]}$. In such a case, provided the date [n] is not too remote from the present, it seems clear that the investor will decide to retain his cash. But this will imply, of course, that the national aggregate investment-flow will be lower, over some period of the immediate future, than it would have been in the absence of any blueprint such as $B_{[n]}$.

Even if it seems to the investor that the best alternative to $B_{[n]}$ which will be open to him at date [n], should the answers to the special questions turn out unfavourable to $B_{[n]}$, will be less attractive than $A_{[1]}$ now is, he may yet decide to retain his cash : for the contingency of a deterioration in his position, such that he will be offered at date [n] a worse instead of a better opportunity than $A_{[1]}$, may be out-weighed in his mind by the contingency of a great improvement. On the other hand it is possible than even if the best alternative to $B_{[n]}$ is actually *better* than $A_{[1]}$, he may yet decide to construct $A_{[1]}$ ; for the length of the period from date [1] to date [n] is also, as we shall see, a factor in his decision. If this period is short in comparison with the useful lives of plants such as $A$ and $B$, and if, for example, as will frequently be the case, he expects $A_{[n]}$ to be not less attractive than $A_{[1]}$, then the existence in his mind of a blueprint for deferred construction which is *contingently* very much more attractive than $A_{[1]}$ will cause him to retain his cash.

Why should a decision to invest now in $A_{[1]}$ preclude the investor from investing a little later on in $B_{[n]}$, if by then he has come to prefer the latter, even supposing his cash resources, owned and borrowed together, are only sufficient to pay for one or other of the blueprints at a time ? Could he not sell plant $A$ when it is complete, or obtain a loan secured on it ? This is the heart of the problem of *liquidity*, which consists essentially in the fact that a man can hold with perfect sincerity (and often, as it turns out afterwards, with justice), expectations which imply a far higher value for the plant than he can persuade others to believe in. The focus gain which he himself assigns to $A$ may be larger, and the focus loss smaller, than the corresponding ones in the mind of any potential buyer of plant $A$. If in such a case he yet insists on selling $A$, he will make, according to his own expectations and corresponding valuation of $A$, a loss, and this may well offset the superior attractiveness which $B_{[n]}$ may by then have acquired. Thus, if having constructed $A$ he finds when date [n] is reached that $B_{[n]}$

has become a very attractive proposition, it may seem better none the less to hold on to $A$: *but he will regret not having retained his cash.* If at date [1] he foresees that this position may arise at date [n], he will decide to retain his cash.

The mere existence in the investor's mind of a blueprint for deferred construction whose focus gain and loss are both very high in relation to its construction cost will not by itself constitute an inducement to him to retain his cash. For in regard to many such blueprints he will attach high potential surprise to the occurrence of any considerable decrease of the focus-loss at or before the construction date. He may often feel sure that no extra light will be thrown on the prospects of a given blueprint except in the actual course of using the plant after it has been constructed. He may have hypothetical future events in mind which could occur without surprising him, and which would reduce the focus loss, but by an amount insufficient to render the blueprint attractive. In the nature of things, events must be rare which can seem so comprehensively to block up all paths by which misfortune might strike the enterprise in the course of its career, that they reduce the focus loss to nothing. But there are certain classes of "experiment", such as elections, parliamentary voting on particular pieces of legislation, lawsuits, and harvests, the date of whose occurrence can be known in advance, and whose outcome can, if it is of the right kind, defend the enterprise against some of the hazards whose impact would be most disastrous and would otherwise carry no potential surprise. If the question, the answer to which is expected to throw light on the prospects of a blueprint, is one whose answer will be some value of a continuous variable, then $y_{[1]\,[n]}$, $B_{[n]}$ may contain not merely a finite set of different hypotheses as to the form of potential surprise function which will be assigned to $B_{[n]}$ at date [n], but an infinite set, each member of which will, of course, imply a different focus loss or gain, and in this case the *amount of the decrease* of either of these latter which can be imagined to occur at date [n] will itself have to be looked on as a continuous variable associated with a varying degree of potential surprise. The idea of a large decrease of the focus loss will, of course, be more interesting to the investor than that of a small decrease; but beyond a certain size each larger hypothetical decrease will carry a higher degree of potential surprise. Thus in such cases we can identify and speak of the *focus decrease of the focus loss.* When the investor is deciding whether the prospect of a decrease of the focus loss of a particular blueprint for deferred construction is a sufficient inducement to him to retain his cash, the particular amount of decrease which concerns him is this focus decrease. We shall refer to that one of the hypotheses contained in $y_{[1]\,[n]}$, $B_{[n]}$ which corresponds to the focus decrease as the focus-hypothesis of $y_{[1]\,[n]}$, $B_{[n]}$. It will be seen that if the investor is to attach low potential surprise to the occurrence of a given decrease of the focus loss of $B_{[n]}$ at date [n], this requires him to attach low potential surprise to *two* hypotheses: (1) that a suitable question bearing on the prospects of the blueprint will be answered at date [n], and (2) that the answer will be such as to effect this amount of decrease in the focus loss. Now, as we have seen in Section IV, if the investor attaches some positive degree of potential surprise to the *non-answering* of such a question on the date [n], it is not logically possible for him to attach any positive degree of potential surprise to the idea that the question, if answered, will *not* effect a decrease of the amount in question, since he is bound to attach *nil* potential surprise to the hypothesis that *no* decrease of the focus loss will occur, as otherwise the lower branch of $y_{[1]\,[1]}, B_{[n]}$ would not have the form which it has. From this it follows that, although frequently in practice, as in the case of a Budget or election, he may attach some positive degree of potential surprise to the *non-answering* of the question on the date [n], this will not make the inducement to retain cash any stronger than it would be if he merely attached *nil* potential surprise to the *answering* of the question at that date; for when some given result depends on the fulfilment of several conditions, then the non-attainment of the result can only carry positive potential surprise if the non-fulfilment of *all* the conditions carries positive potential surprise. It may be worth while to enlarge on this point for a moment. In general, it will be seen, if we attach positive potential surprise to the *non*-occurrence of some particular result, this implies, of course, that the *occurrence of all alternative results* carries positive potential surprise: in the case of a continuous variable $x$, for example, $y\,(x)$ might have the form shown in Figure $3_A$ or $3_B$, amongst an infinite variety of others:

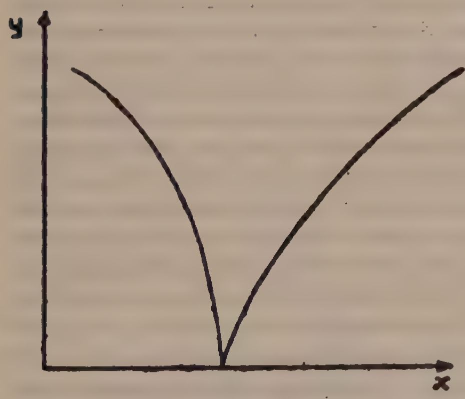

FIGURE $3_A$

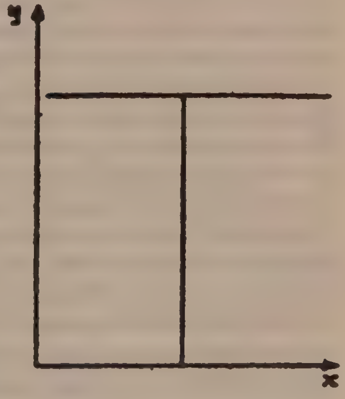

FIGURE $3_B$

The degree of potential surprise attached to the non-occurrence of any given result is evidently identical with the least degree of potential surprise attached to any alternative result.

The strength of the inducement to retain cash, arising in the way we have described in this section, will depend on four sets of factors :

($\alpha$) The focus gain and loss of $A_{[1]}$ ; the focus gain and loss of $B_{[n]}$ according to the focus-hypothesis of $y_{[1]\,[n]}$, $B_{[n]}$ ; and the focus gain and loss of that blueprint, say $C_{[n]}$, which would become the most attractive one in the investor's mind for construction at date [n] if the questions should be answered in a sense *unfavourable* to $B_{[n]}$.

($\beta$) The length of the interval between date [1] and date [n].

($\gamma$) The degree of potential surprise attached to the hypothesis that the questions will be answered not later than date [n].

($\partial$) The amount by which the value we attribute to $A_{[1]}$ if we assume that its focus gain will be realised exceeds the price for which plant $A$ could be readily sold at date [n].

Regarding ($\alpha$), the investor may have in mind as an alternative to $B_{[n]}$, in case the latter's focus loss fails to decrease at date [n], a blueprint $C_{[n]}$ whose focus loss would be greatly reduced, and $C_{[n]}$ thus rendered extremely attractive, by that same set of answers to the special questions which would reduce the focus *gain*, instead of the focus loss, of $B_{[n]}$. This circumstance would, of course, tend greatly to strengthen the inducement to retain cash. Next it is clear that the *length of time* he must wait in order to know the answers to the special questions is an influence on the inducement to wait. For, by assumption, there is something he would prefer to do with his cash rather than lend it at fixed interest. To wait therefore involves an item on the debit side, a cost, and the longer the prospective time which it will be necessary to wait, the stronger the temptation to choose the other alternative, the immediate construction of $A_{[1]}$. Regarding ($\gamma$), we have seen that the investor cannot attach any positive degree of potential surprise to the *non*-occurrence at date [n] of a decrease in the focus loss of $B_{[n]}$ : at best, he can attach *nil* potential surprise to the *occurrence* of a decrease. This requires him to attach *nil* potential surprise to the hypothesis that the questions will be answered on date [n]. But there may still be some degree of inducement to retain cash, even when this hypothesis carries some positive potential surprise. We can say, therefore, that the inducement to retain cash will be stronger, the more nearly the potential surprise attached to the idea that the questions will be answered not later than date [n] approaches zero.

Clearly we cannot explore all the possible combinations of circumstances which can face the investor in making a choice between constructing a certain blueprint now and retaining his cash in view of the contingency that some other much more attractive opportunity may present itself a short time hence. What we have done in this section is to isolate the essentials of such a situation. It will be understood that throughout this discussion the word " attractive " is used in a purely subjective sense. An investment-opportunity, or blueprint, is something which exists in the investor's mind. Its characteristics and qualities are thoughts, and not something which can be observed without reference to the individual.

## VI

We have now to consider how a particular blueprint for immediate construction, say $A_{[1]}$, whose focus outcomes according to $y_{[1][1]}$, $A_{[1]}$ are in themselves highly attractive, may be rendered unattractive by the fact that one of the hypotheses contained in $y_{[1][3]}$, $A_{[1]}$, and carrying *nil* potential surprise, implies a much lower focus gain than $y_{[1][1]}$, $A_{[1]}$. To explain this proposition we must consider the inherent structure of enjoyment by anticipation.

If we attach low potential surprise to the future occurrence of an event which, when it occurs, will give us pleasure, we can take pleasure in *imagining* this event in advance. To do this is to get *enjoyment by anticipation.* Since to enjoy by anticipation is itself a pleasurable act, and can itself be imagined in advance, it can give rise to a secondary enjoyment by anticipation, in which we enjoy the prospect of enjoying the prospect of a pleasure-giving event. And there can evidently be a tertiary, etc., enjoyment by anticipation. To make matters precise, let us consider an arbitrary short period called a “ day ”, and let us call the day on which the event itself is due to occur day n, let the preceding day be day n – 1, and so forth. Then on day n – 1 the individual will enjoy the direct anticipation of the event itself. On day n – 2 he will enjoy, first, a direct anticipation of the event itself, though at a greater distance in the future, and therefore less vivid and intense, than that which will be felt on day n – 1 ; and second, he will enjoy looking forward to the pleasurable feelings which he is going to experience on day n – 1. On day n – 3 this two-fold enjoyment which is going to be experienced on day n – 2 can itself be looked forward to, as well as the feelings which are going to be experienced on day n – 1, and the event itself ; and so on. Each day added to the intervening period has two distinct and opposite effects on the total degree of enjoyment by anticipation felt by the individual at the moment from which he is looking forward : first,[1] it will remove the event itself, and also day n – 1, day n – 2, etc., one day further off from the present moment, and hence the intensity of enjoyment from looking forward to each of these days will be reduced. Secondly, it will add an extra day to those future days on which pleasurable feelings are going to be experienced, and which can therefore themselves be looked forward to with pleasure. This second effect has not, so far as I know, been anywhere discussed heretofore ; but the first effect is a familiar idea in economic theory, since it has no doubt been in the minds of all those who have used the concept of "impatience" in discussing the rate of interest, and this first effect will be generally admitted to be often strong and important. Hence if we can show that the second effect is sometimes powerful enough to overcome the first effect, the importance for theory of the second effect will have been established. Now it may be that the first of these effects is always stronger than the second, so that *any* lengthening of the intervening period, however short that period was, causes a net reduction of the intensity of present enjoyment by anticipation. But there seems to be strong evidence that this is not so. Introspection and the testimony of others indicate that a prospective enjoyable event can be *too near* in time for the intensity of enjoyment by anticipation due to it to be a maximum. Suppose we know that a holiday is due to us at some unspecified time in the coming twelve months. Then if we are suddenly informed that it is to begin to-morrow, we shall feel that we have lost something by the shortness of the notice, even supposing that all our plans and preparations have been made. In greater matters I believe this factor is correspondingly more important. For example, a man who might be prepared to venture a quarter of his fortune on an expedition to prospect for gold, whose outcome will not be known, perhaps, for years, might decline to risk it on the turn of a card, though the focus gain and loss were the same in both cases. However, the question whether the first of the two effects is or is not stronger for all lengths, however short, of the period which separates the present moment from the date of the anticipated event, is irrelevant for our present purpose. What we can say, on the basis of pure logic, is that an investor will feel that a blueprint for immediate construction, of given focus gain and focus loss, whose outcome will be known at a specific future date, say $T$, will be rendered less attractive by the presence in his mind of some question, due to be answered at a much nearer date than $T$, which question if answered in one particular way would greatly reduce the focus gain of the blueprint while leaving its focus loss unchanged. For the possibility, carrying low potential surprise, that the question might be answered in that way, will cause him to fear the loss of part of the period during which he could otherwise hope to enjoy by anticipation a favourable outcome of the venture. When he begins construction of the blueprint, he looks forward to being in "enjoyable doubt" (less enjoyable, of course, than certainty of a gain equal to the focus gain, but more enjoyable than certainty of a very small gain) as to its outcome during the whole of a period of years. It will be a serious impairment of the attractiveness of the blueprint if he feels that he would not be surprised to learn something one month hence which will greatly reduce the focus gain: for this contingency would transform his prospective state of mind, for the rest of the period up to date $T$, into one of "disagreeable doubt".

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[1] We shall refer to these as the first and the second effects. These “ effects ” merely describe the comparison of what a person would feel when looking forward to a given event $n$ days ahead with what he would feel when looking forward to the same event $n + 1$ days ahead. The “ cause ” of these “ effects ” is merely the shift of our attention from one idea to another, and the “ effects ” are therefore, of course, simultaneous.

## VII

If the argument of the preceding sections is accepted, it is clear that the approach of some event, such as a Presidential election or an international conference, some conceivable outcomes of which are widely different from each other and would have extremely different effects on the valuation of a specific investment blueprint, will tend to make the aggregate investment flow less than it would otherwise have been. There will be a *general* tendency amongst investors to wait and see. At a date when such an event is still remote, most of the investors who are then making such comparisons as that between $A_{[1]}$ and $B_{[n]}$, which we described above, will feel that the waiting time would be too long, and will each decide to construct at once the plant representing his $A_{[1]}$. But at a date nearer to the event, most of them will feel it worth while to wait and see. Thus the strength of the tendency for investment to be discouraged will continually increase as the event draws nearer, and then jump discontinuously to zero at the moment when the event has just occurred.

It follows that such an event when it actually occurs may release a large number of investment-decisions, *even though it has not lessened the focus-loss nor raised the focus-gain of any blueprint in the mind of any investor.* Blueprints for immediate construction will no longer have to compete in the investor's mind with the contingency that opportunities of still higher promise may arise at a near future date ; and their attractiveness will not be decreased by the fear that a near future date may bring an abrupt reduction of their focus gain. Thus many blueprints such as the one which in Section V we labelled $A_{[1]}$ may begin to be constructed.

## VIII

An actual occurrence which causes surprise proves that the individual's structure of expectations[1] either contained a misjudgment or was incomplete. Either the event was included amongst his hypotheses but excluded from the inner group, where clearly it ought to have been, or else it formed no part of any hypothesis.[2] If his exclusion of this event from the inner group was wrong, so may be that of other hypotheses still to be tested, and he must consider again his other judgments of this kind. If the event was something entirely

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[1] i.e., the whole assemblage of sets of variants in his mind.

[2] I believe the distinction may be important between these two types of surprising events, which I propose to call *counter-expected events* and *unexpected* events :

| | |
|---|---|
| Counter-expected event : | an hypothesis which has been considered and to which as a consequence of this examination a high degree of potential surprise has been assigned. |
| Unexpected event : | a contingency which has entirely escaped attention, which has never entered the individual's mind, and has formed no part of any hypothesis. |

A person's structure of expectations may be more completely demolished by an unexpected event than by a counter-expected event : the former reveals not merely a misjudgment but the fact that the individual has been unaware both of the essential features of the situation and of the existence and extent of his ignorance.

unthought of he must consider a mass of new ideas which it will generate concerning the future course of events. Thus an important surprising event will require him more or less to create afresh his structure of expectations. Such an event will cause his whole existing set of judgments, by which he has assigned to each hypothesis its particular degree of potential surprise, to become a dead letter.[1] He cannot instantly substitute for this old set of judgments a new set emerging from a thorough examination of the surprising event in all its bearings and implications, for this examination will take time. But in the nature of things it must be possible for him at any moment to answer the question whether the actual realisation of such and such an hypothesis would surprise him. It follows that he will have in mind, during the interregnum between the abandonment of one set of fully considered judgments and the establishment of a new one, some set of provisional judgments. This must, in the nature of the case, admit of a wide allowance for events, and values of variables, which formerly he would have labelled as potentially surprising, but which he cannot now condemn offhand. Thus the impact effect in his mind of a major surprising event[2] will include a great precautionary increase in the focus gain and focus loss of each one of the blueprints he has in mind. But at the moment when he makes these increases, he will be able to look forward to a moment, that moment namely when he will have completed the examination of the significance of the surprising event, when many or all of these focus gains and focus losses will again have been reduced; not, of course, in general back to their original positions, but to new levels higher or lower than their original levels but lower than their provisional levels. He will, at least, attach very high potential surprise to the supposition that most of them will *not* by then have been thus reduced.[3] Thus, if at the moment when a surprising event has just occurred, he looks ahead to the moment when his examination of this event will have been completed, he is looking ahead to a future date at which the answers to special questions, in the sense we have given to this phrase above, will become known to him. Thus the occurrence of a surprising event will give him the same incentive to " wait and see " as the approach of a date when the outcome of special " experiments ", such as elections or harvests, will be known. He will be tempted to hold off from embarking immediately on the construction of any blueprint $A_{[1]}$ for two reasons:

(1) in case some other blueprint may emerge from his process of examination with a more attractive pair of focus outcomes

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[1] These existing judgments will not necessarily be altered as a consequence of his studying the implications of the surprising event, but until this examination has shown whether or not any change is called for, their authority will be suspended.

[2] By a major surprising event I mean one which is both very surprising to the individual and highly relevant to his judgments: that is, a very surprising change in some very important circumstance.

[3] Since *both* its focus outcomes will be reduced, he cannot tell anything as to whether any blueprint will be rendered more attractive or less attractive by the process of examination: either can happen.

than those assigned to $A_{[1]}$ by his provisional judgments; and
(2) in case his new set of fully-considered judgments assigns to $A_{[1]}$ a much less attractive pair of focus outcomes than those assigned to it by his provisional judgments.

The former possibility will be made more insistent to his mind by the fact that the precautionary increases of focus gains and focus losses will, by their nature, have been such as to *level up* the attractiveness of different blueprints. These precautionary increases will have been generous in all cases, and more or less determined by a factor common to *all* the blueprints, namely the need to make sure that these increases shall be large enough. The individual will tend to push them all up to some common high level.

Our main conclusion from the argument of this Section is as follows: One effect of an event which causes surprise will be to heighten at first the attractiveness of liquidity, that is, of deferment of choice of a specific blueprint, and discourage the immediate construction of equipment. If a large number of investors are thus affected by the same event, the aggregate investment flow in some period closely[1] following this event will be lower than it would otherwise have been.

The time occupied by the process of examination may be considerable. The sorting-out and assembling of fresh impressions, the gradual evolution of new ideas, the tracing-out of all the bearings of the event on what was contemplated before, and the canvassing or waiting for signs of other people's reactions to the same event, will make an arduous process not completed in a day or two. We shall call this process the assimilation of the event into the structure of expectations. To this process may be added a *testing* of the new set of judgments by waiting to see if more events will come along such that *they would have been surprising had they occurred before the event which occasioned the process of examination, but are not so now that this event has been assimilated.*

If an event, after its meaning has been assessed, *increases* a focus-gain or a focus-loss, then by the definition of these latter it must have been surprising. A hitherto unthought of event, even though it turns out, after assimilation into the structure of expectations, to *decrease* a focus-gain or a focus-loss, will also at its occurrence have been surprising. And, as we have seen in Section V, it is possible for a hypothetical decrease of a focus-gain or loss, when this decrease exceeds a certain size, to carry a positive degree of potential surprise, and such a decrease if it occurred could therefore be the outcome of a *counter-expected* event. But many of the events which *decrease* a focus-gain or loss will be non-surprising: they will be some of those very events which the investor had in mind in assigning *nil* potential surprise to those profit-outcomes of the venture falling within the inner range; such events will be assimilated easily and quickly into the individual's structure of expectations, and he will be in as good a position to make a decision immediately after such an event as before it. Thus we find that there is an important *asymmetry* between investment-stimulating and investment-depressing events. Investment can be stimulated, after the assimilation of the event, by either an increase of focus-gains or a decrease of focus-losses. But only in the latter case will the assimilation and therefore the effect be immediate. In the former case the *impact effect* will be depressive. Investment can be depressed by either an increase of focus-losses or a decrease of focus-gains, and the effect in *both* these cases will seem to be immediate, though in fact the downward movement following an event which is going to increase focus-losses will be due at first to the mere " stand-still " effect of the surprising event. This asymmetry seems at least partly to explain why the decline of investment and employment after a boom is usually more abrupt and rapid than their recovery after a slump.

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[1] Closely, rather than immediately, since plans for the immediate future can only be altered at high cost.

## IX

Our final task will be to try to suggest a possible line of attack on the central problem of all, the question by what means and process does the individual select, for each different course of action that seems open to him, those hypotheses as to the outcome, or the path which the future course of events will follow, which are to carry less than indefinitely high potential surprise, and are therefore to provide his basis for comparing the attractiveness of this course of action with that of any other ? When we think of the individual as being free to choose amongst a set of different courses of action, we mean strictly speaking that he can choose amongst the respective initial moves, or " gambits ", in these different plans. He will not usually determine in advance a unique move for every stage of his plan, regardless of what shall have been the reactions of others to the preceding steps.

We shall regard the individual's power to choose his own immediate next action, or gambit, as a power to " choose " the immediate situation. Thus we think of past history as a series of situations culminating in a " present " situation which hangs in suspense, as it were, until the individual has chosen his gambit. When, for the purpose of weighing the attractiveness of a given course of action, he has provisionally done so, he has a complete series of known situations covering some period of time measured backwards from the present moment, and the *immediate* future, each of these situations being " known " in the sense that some of its attributes are uniquely determinate (others, of course, indeed the majority, will be forever unknown to any one individual), and his task is to determine some set of hypothetical situations, as few and mutually alike as possible, amongst which the next, future, situation will be found.

In seeking a basis for some conjecture as to how he proceeds, we can perhaps get a hint from remembering what is the first and fundamental operation of all inductive science: classification. Thus the first of our basic concepts is that of a *situation-type*. By this I mean simply a specification of a state of affairs conceived to exist at a point of time, the purpose of such specifications being to serve as a means of classifying actual situations according to their degree of resemblance to one or other of a set of situation-types. What we mean here by resemblance will be touched on below (page 123). Instead of the term situation-type we shall use the word *symbol*, since in the mental process we are going to describe, the individual treats a situation-type as equivalent to any of the actual situations which he classifies as belonging to it, and after once performing the classification, thenceforth concerns himself with situation-types alone. Thus the latter *stand for* actual situations, and we shall call them symbol-situations or merely *symbols*.

The apparatus by means of which a person answers the question " If I choose such and such an immediate action, and thus complete the ' existing ' situation, whose nature will then be fully determined, what will be the next following situation ? " will consist according to our hypothesis in a finite set of symbol-situations and a system of rules prescribing a correspondence between sub-sets of these symbols. When the members of any sub-set of these symbols are taken in a given order, we shall have the left-hand side of what we may call an expectation-equation. The system of rules will then prescribe one and only one[1] sub-set of symbols, considered without regard to the order of its members, as the right-hand side. The reason why the symbols on the left-hand side must be taken in a given order is that the situations of history occur in a given time-order, and their sequel would often be different if their order were different. The reason why there is no question of order on the right-hand side is that the situations there are *rival hypotheses*, all referring to the same future point of time. In this conception the flow of history is thought of as consisting in the passage from one to another of a series of situations, the latter being conceived in such a sense that there will be a finite number in any time interval of given length. Thus history is thought of as resembling a cinema film, wherein each of the " still " pictures, which when traversed in succession yield the movement of events, corresponds to one particula point of time. The process by which the individual will form his expectations concerning the next future situation will be as follows: He will consider a certain period of the past, extending back from the present moment,

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[1] This does not and, of course, cannot imply that there will be a *different* sub-set on the right-hand side for each different order of each different sub-set on the left-hand side, for even if we count only the permutations which have no repetitions of symbols,

$$\sum_{r=1}^{n} {}^{n}P_{r} > \sum_{r=1}^{n} {}^{n}C_{r},$$

and when we allow for repetitions of symbols (which will very frequently occur) on the left-hand side, but not, of course, on the right-hand side, the disparity is even greater.

and formulate the history of this period so far as he knows it into a finite series of situations, including one representing the existing situation as determined by his own choice of an immediate action. He will then *classify* each of these actual situations as equivalent to one or other of his symbol-situations, and thus obtain a finite sub-set of symbol-situations in a given order. This by hypothesis will yield some other specific sub-set, regardless of internal order, of symbol-situations, and this latter sub-set constitutes his set of hypotheses concerning the situation which will come into being next. Within the sub-set there may be a sub-sub-set whose members carry *nil* potential surprise, while the remaining members of the sub-set carry it in increasing degree according as their resemblance to the most similar member of the sub-sub-set is less.

The above is the formal frame of our hypothesis. To develop this into something like a complete theory, we must try to answer the following questions :

1. By what criteria will an actual situation be assimilated to, i.e., classified as belonging to, some particular symbol-situation ?
2. How does the individual decide how long a past period to take as the basis of his expectations ?
3. How will an individual's system, consisting of his set of symbols and his rules for combining them (i.e., for assigning to each permutation one particular combination), come into being and develop ? How does he derive the rules which solve the expectation-equations ?
4. What will the individual do if, when he comes to add a situation which has just slid, as it were, into the past, on to his chain of past situations, its symbol proves *not to be a member* of the sub-set on the right-hand side of the equation of which that chain, before the addition of the new situation, formed the left-hand side ?
5. Does this conception imply that, from a knowledge of a few past situations, an answer can be obtained specifying the situation which will exist on any future date however remote ?
6. Is it possible for an individual to build up and retain in his mind, and have always available for application, a system of rules comprehensively capable of dealing with the vast number of possible permutations of symbols which can occur on the left-hand side of his expectation-equations ?
7. Are such systems as we have described the same for all people ? If not, are they sufficiently alike for generalisations applying to all or the majority of people to be built ?

No proper discussion of these points can be attempted here, but we shall touch on each of them briefly, in order to indicate a possible line of attack where it may be thought that our theory encounters a difficulty.

Let us represent a situation by a point in m-dimensional space. This is quite a convenient notion, as each of the orthogonal co-ordinates of this point can then be thought of as standing for one of the mutually independent features, attributes, or elements, whatever we prefer to call them, of the situation ; for example, the price of some commodity, the voting-strength of a political party, the size of a harvest. A change in any of these magnitudes will give us a different value for the co-ordinate representing it, and thus a different point, standing for what will, of course, be a changed situation. We can specify such a point by a vector, say $[r_1 r_2 \ldots r_m]$, wherein the elements $r_1 r_2, \ldots$ must always be retained in a fixed order so that we may know to which of the co-ordinates each of them corresponds. An actual situation can, of course, only be very imperfectly known to any one person. He is sure to be ignorant even of the existence of some of its most important features, for example, the ideas and intentions of influential persons, and amongst those of its co-ordinates which he is aware that he ought to take into account, the magnitudes of many will be unknown to him. Let the vector $[R_1 R_2 \ldots R_m]$ stand for one of his symbol-situations. This will prescribe the number and nature of the co-ordinates to be taken into account, and the corresponding co-ordinates of an actual situation can be represented by $[r_1 r_2 \ldots r_m]$. The individual may find that he knows nothing of the values of $r_1 r_2$ and $r_3$, but that the values of the remaining elements lie within certain distances of the corresponding elements $R_4 R_5 \ldots R_m$ of the symbol-situation. He then assimilate the actual situation to the symbol-situation.

When the individual is setting up the left-hand side of his equation, the series of successively earlier situations stretching back into the past, available for this purpose, is evidently indefinitely long. We have supposed him to take some finite, in practice quite small, number of these, classify them into situation-types, and, treating the result as a single entity, i.e., a set of given symbols in a given order, obtain from it by reference to a system of rules a set of rival hypotheses as to the next future situation, this entire set of hypotheses considered as a whole constituting the solution. Now suppose he has taken $N$ past situations and obtained a particular set of hypotheses as the answer. And suppose he now takes one more, still earlier, situation : will this give him a different set of hypotheses ? If we think of actual attempts to interpret past history, we are bound to answer that it may. For example, if we are given that two successive harvests in a given region were each of a hundred million bushels, and are told nothing else, our conjecture as to the next harvest will be different from what it would have been had we been told that the next earlier harvest had been of two hundred million bushels. But how then does the individual decide when to stop taking more and more past situations, if each additional one may give him a different answer ? The principle we must adopt here, I think, is that, given the character of the most recent situations, the degree to which the interpretation of them need be modified or coloured by the nature of earlier situations will decrease according as the latter are more remote. As the familiar saying goes, "It will be all the same a hundred years hence". However, so far as more remote past situations can contribute to our understanding of the recent past, the effect of taking them into account must surely be to narrow the range of our uncertainty. It seems reasonable to suppose that the longer the chain of past situations we take, up to a point, the *more compact* will be the set of hypotheses obtained. By "more compact" we mean either "less numerous" or "having its mutually most dissimilar members more similar" or both these things. The individual will lengthen the chain of past situations up to the point, which in practice will not be very remote, where the associated increase in compactness becomes negligible.

How will a person's system, consisting of his set of symbol-situations and his rules for combining them (i.e., for obtaining solutions from them), come into being? This system, as it exists at any moment, must evidently be the product of a process of growth which has gone on during his whole life up to the moment in question. At an early stage it will comprise only a few symbols, and sooner or later he will encounter a real situation which is inordinately difficult to assimilate to any of his existing symbols. He must then introduce into his system a new symbol, and prescribe for it rules of combination which will yield a determinate set of symbols for the right-hand side of any expectation-equation into whose left-hand side this new symbol is introduced; and, of course, there must be a determinate "right-hand side" for every one of the possible different permutations of time-order in which the new symbol can occur.

What will the individual do if, having solved an expectation-equation for the situation at date [n], he finds when date [n] arrives that the actual situation of that date is not a member of the set of hypotheses obtained as the solution of the equation? He may decide that some of the actual situations on the left-hand side of the equation must be assimilated to different symbols. But if they all appeared to be easily and unambiguously assimilated to the symbols first chosen, he may be impelled to modify his system of rules. In such a way the rules of combination, as well as the set of symbols, could be gradually developed.

We may here notice that the individual will be guided and constrained somewhat in his choice of symbols to which to assimilate his actual past situations: the series of symbols he obtains on the left-hand side must be *internally self-consistent*. Suppose, for example, that his set of symbols[1] is $[S_{[6]}\ S_{[7]}\ S_{[8]}\ S_{[9]}]$ where we again denote fixed, specific dates by numbers in square brackets, these dates being in this case *past* dates. Then for the sake of logical consistency it is necessary that the set $[S_{[6]}\ S_{[7]}]$ when standing as the left-hand side of an equation should yield as the right-hand side a set of hypotheses which includes the symbol which appears as $S_{[8]}$ in the full series. Similarly $[S_{[6]}\ S_{[7]}\ S_{[8]}]$ must give a right-hand side which includes $S_{[9]}$.

Does our hypothesis imply that, knowing a few past situations, a person can compute, by successive steps, what will be the situation at *every* future date ? No. For the succession of answers, referring to more and more distant future dates, will be rapidly " divergent ", since *every* member of the set of hypotheses obtained as the right-hand side of the equation for future date [n] must be used in turn as the last symbol on the left-hand side of the equation for date [n + 1], and so on. Thus if the solution for date [n] contains $M$ hypotheses, and each of these when used as the last symbol on the left-hand side for date [n + 1] gives a solution with $M$ hypotheses, there will be $M^2$ hypotheses altogether for date [n + 1], $M^3$ for date [n + 2] and so on. Thus it will not be worth while to go very far ahead.

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[1] Since the left-hand side of an expectation-equation is a set of symbols in a given order, we can appropriately represent it by a vector. To avoid confusion with the vectors we employed to specify the character of situations, we shall henceforth treat the latter as column-vectors.

We have suggested that the individual will have a completely comprehensive set of rules, able to give him the right-hand side of *any* expectation-equation which can be set up with the symbol-situations which exist in his system. That is to say, any permutation of any subset of the symbols which at present exist in his system, when set down as the left-hand side of the equation, must be capable of solution by reference to his rules. But this does not mean that he will have to create in his mind, and retain for reference, an immense number of separate, unsystematised results, in a process like learning to spell, that is, to attach separate, unsystematised meanings to arbitrary permutations of letters. On the contrary, I conceive of the rules as analogous to such a system of rules as those for the multiplication of matrices. In matrix algebra, multiplication is non-commutative, so that, for example, the product-matrix $BA$ of the two matrices $B$ and $A$ is in general different from the product-matrix $AB$ of the same two matrices, supposing that both products exist. But the rule for multiplication, once established, enables us to obtain the product of any set of matrices arranged in some specific order, provided they are conformable for such product, simply by performing a series of ordinary multiplications and summations of ordinary scalar numbers. Thus we conceive of the individual as having a compact *technique* for the combination of symbol-situations, which can be applied to any desired permutation of symbols.

The last of our questions asks whether such a system as we have adumbrated would differ in its concrete forms and details from person to person, or whether there is in the nature of things something which would lead them all to develop the same system ? Since the personal experience of every individual is different from that of every other, and their minds are also different through hereditary causes, it seems perfectly clear that the systems will differ from each other in detail, and often in very important ways. But the record of history " in the large ", that is, of those events which touch everybody in an obvious way, is roughly the same for everybody. And since this record is an important part of the raw material for building up such a system as we have sketched, it seems possible that the systems developed by different individuals may show sufficient uniformities for generalisations to be possible.

## X

We shall call a sequence of non-future situations which, in the framework suggested in the last section, we suppose to determine, regarding the next future situation, the set of hypotheses to which an individual will attach nil or low (as distinct from indefinitely high) potential surprise, a *gnomon-configuration* or *gnomon*. It will consist of a sequence of vectors each specifying the situation at one point of historical time, as that situation is now known to or conceived by the individual. The latest of these points of time will be the individual's viewpoint (Section 4 above), that is to say, the sequence includes the existing situation. Each vector will be a set of co-ordinates written in a column, say

$$\begin{bmatrix} R_1 \\ R_2 \\ \cdot \\ \cdot \\ R_m \end{bmatrix}$$

and each of these co-ordinates will stand for one element in the set of elements or variables which the individual regards as composing the main structure of every situation. A gnomon can thus be thought of as a matrix, say

$$\begin{bmatrix} R_{11} & R_{12} & \cdot & \cdot & R_{1n} \\ R_{21} & R_{22} & \cdot & \cdot & R_{2n} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ R_{m1} & R_{m2} & \cdot & \cdot & R_{mn} \end{bmatrix}$$

of n columns or situations, and m rows or *time-series of the values of* m *different variables.* We shall not here attack the problem of the nature or detailed specification of the correspondence[1] on gnomons to sets of hypotheses. But it happens that we can suggest an answer to a simpler question which, for the purpose of applying our theory to the theory of investment, can take the place of the more fundamental one: Is there any *formal and general* characteristic of all gnomons, varying in degree between them, such that it can be pointed out and measured for any given gnomon the values of whose elements are given, *without our knowing what these elements stand for*,[2] and such that the " value " or " level " or " classification " (as high, low, etc.)

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[1] As we pointed out in Section IX, there cannot be a one-to-one correspondence between gnomons and sets of hypotheses, since the former outnumber the latter, but we assume a correspondence on gnomons to sets of hypotheses. That is, each gnomon determines a unique set of hypotheses, but not *vice versa*.

[2] We seek a characteristic of gnomons *in general*, as a class. A characteristic would not be a *formal and general characteristic of gnomons as such*, if we needed to know the concrete meanings belonging to the elements of each gnomon before we could classify it.

of this characteristic will tell us whether the set of hypotheses, corresponding to this particular gnomon, is likely or unlikely to induce the individual to invest ?

The idea of constructing now a particular blueprint will seem highly attractive, if the investor believes that the demand for the services of this type of equipment will be very much stronger[1] in relation to the supply of the equipment, or of the resources for making it, in the next future situation than in the existing situation. For this belief will mean that the cost of constructing the blueprint now will be low, but that the present value attributed to it by the individual's view of the future will be high. In order that the two successive situations (existing and impending) may appear to stand in this relationship to each other, the investor must assume that some one or more of the variables composing the situations will show a wide change of value between the two situations[2] ; that is, since the length of the period separating the two situations is regarded as fixed, he must assume that the variable in question will undergo a very rapid change over the immediately-ensuing period of the future. It must also be assumed that over this period, between the existing and the next future situation, no changes of other variables will occur such as to cancel the effect of the movement of that variable or group of variables which implies a strengthening of demand. Can we select any one type of gnomon as making it specially plausible that favourable[3] changes will indeed occur and that no unfavourable changes will occur ? I suggest that this will be specially plausible if the investor has seen a favourable movement of a single main variable occurring alone in what we may call *experimental conditions*, and that *these conditions are still operating*. By experimental conditions I mean a state of affairs where all the other main variables, except this single active one, remain constant. For if there has been such constancy of all but one variable over the immediate past, then (*a*) the constancy of all the other variables is evidently compatible with rapid change of the one in which the investor is interested ; and (*b*) certain factors or conditions have been operating such as to give rise to the movement of this variable, and these particular conditions, which brought about the rapid movement which the investor has just seen occurring in the immediate past, have remained in force and are still operating at the investor's viewpoint : for *all* the other main variables have remained constant, and hence so also have their derivatives of first and all higher orders ; and thus if the movement of the single active variable depends only on the values, whether synchronous or lagged, of some or all of the other variables themselves or of their derivatives, or the relations between any of these magnitudes, it follows that since all such magnitudes, whether relevant or irrelevant to the determination of the path of the active variable, have remained constant, the *relevant* conditions must have remained constant, and the factors generating the favourable movement of the active variable, though we cannot specify nor identify them, are therefore still in force. Thus if we can assume that there will not be any *discontinuous* changes of the conditioning variables, a continuation of the movement of the active variable for some short period at least may be looked for. Moreover, since this particular change is the only one which has been occurring, there is no special reason to suppose that other changes, amongst which there might be ones adverse to the particular blueprint whose merits are being assessed, will begin in the immediate future.

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[1] I.e. that the demand curve will have shifted upwards, so that more will be bought at each given price.

[2] That is to say, that successive elements of some one or more rows of the gnomon-matrix will show large (positive or negative) differences.

[3] We assume that the investor will select for intensive consideration, out of a large number of widely different types of blueprint appropriate to widely differing products or methods, that type which is best adapted to take advantage of the movements of variables which he expects. But a movement or set of simultaneous movements which is favourable for one blueprint will be adverse for others. Thus the expressions 'favourable' and 'unfavourable' movements have meaning only in relation to each other, and to specific blueprints.

There is one possible and important departure, which we must recognise, from our assumption that the movement of a variable is determined only by values, first or higher derivatives, or the relations between these, of the other variables : it may be that the subsequent path of the active variable is partly determined by some integral over time of its own past movement ; or, if some of the other main variables are themselves rates of flow, it may be that the path of the variable is influenced by some integral over time of these flows. Such considerations, however, will not destroy the appeal which the rapid movement of a single variable against a background of constancy will make to the investor's imagination : he will interpret it with much greater assurance than a state of affairs where a large proportion of the variables have all been changing at once.

If the view we have been putting forward is accepted, there are two powerful reasons why the type of gnomon here considered may be supposed to give a specially strong inducement to construct the appropriate blueprint. We have seen that it provides a presumption of *persistence into the future* of the recent movement of the one active variable, and some presumption that there will be no abrupt and immediate interference by unfavourable movements of other variables. The investor can thus attach nil potential surprise to a strengthening of demand for the services of the equipment between the existing and the next future situation. But besides this, the absence of movements of other variables over the immediate past increases the attractiveness of the blueprint in another way. Had there been such movements, the individual would have to attach nil potential surprise to their persisting into the future, and their total effect on the next future situation, comprising not merely their direct effect on the demand which will emerge at that time, but also their effect on the expectations which other individuals will then form about the further future, would have been incalculable as to its structure and its degree, and would have compelled the investor to attach nil potential surprise to the hypothesis of a great *deterioration* of the circumstances affecting the value of his equipment. For the multiplicity of simultaneous movements in the past would be the worst possible basis for discovering the influence of individual movements, and even if such knowledge were obtainable it would only apply to the circumstances which *have* existed, and not necessarily to those which will be created when the variables have continued their movements. An individual can grapple to some extent with the problem of what future situation will be created by the rapid movement of a single variable in an environment of constant values of other features : but if all variables appear to be simultaneously shifting at a variety of different speeds, he will feel compelled, from a mere recognition of his own inadequate knowledge of psychic and natural laws and the manifest *insufficiency of time* for him to trace out in his mind every possibility or investigate each individual problem, to make a wide comprehensive allowance for the outcome to be almost anything within a large region of the m-space in which his situations are defined. Thus the type of gnomon-configuration we have been considering will tend to give both a high focus gain and a small focus loss for that particular blueprint which is specially and peculiarly adapted to take advantage of the expected movement of the particular variable in question. The distinctive character of such a gnomon can be summarised by calling it a *high-contrast* gnomon : if we can find some way of rendering the variables comparable, e.g. by expressing each as a percentage deviation from its mean, this attribute can be quantified by taking the ratio of the highest rate of change to be found amongst the variables of the gnomon to the average rate of change of all the *other* variables, all the rates of change being taken without regard to sign. Let us suppose that the gnomon comprises only two situations : then the column describing one of these situations can be subtracted from the column describing the other, and the resulting vector of differences will be the set of rates of change in which we are interested. The ratio of the highest numerical value amongst these, to the average numerical value of the others, may be called the co-efficient of contrast.

Our proposition has been presented in this section in an extreme and hence an artificial form. It may seldom be the case that a rapid movement of a single one of the main variables will be generated by an environment of constancy of all the other variables. But many sequences of situations will approach in greater or less degree this extreme type, and our contention is that the more closely they do so, the more boldly will the investor attack the problem of interpreting them.

# The Problem of Rationality in the Social World

By ALFRED SCHUETZ

THE problem suggested by the terms " rationality " or " rational action " as used in current literature is most certainly central to the methodology and epistemology of the scientific study of the social world. The terms themselves, however, are not only used with many different meanings—and this sometimes in the writings of the same author as, for instance, Max Weber—but they represent only very inadequately the underlying conceptual scheme. In order to bring out the concealed equivocations and connotations, and to isolate the question of rationality from all the other problems surrounding it, we must go further into the structure of the social world and make more extensive inquiries into the different attitudes toward the social world adopted, on the one hand, by the actor within this world, and, on the other hand, by the scientific observer of it.

What is commonly understood by the term " rational action " is best shown by the definition of " rationality " or " reasonableness " given by Professor Talcott Parsons in his remarkable study on *The Structure of Social Action* :[1]

" Action is rational in so far as it pursues ends possible within the conditions of the situation, and by the means which, among those available to the actor, are intrinsically best adapted to the end for reasons understandable and verifiable by positive empirical science." Indicating in his usual careful manner the methodological point of view from which he contemplates his problem, Professor Parsons comments upon this definition as follows : " Since science is the rational achievement par excellence, the mode of approach here outlined is in terms of the analogy between the scientific investigator and the actor in ordinary practical activities. The starting point is that of conceiving the actor as coming to know the facts of the situation in which he acts and thus the conditions necessary and means available for the realization of his ends. As applied to the means-end relationship this is essentially a matter of the accurate prediction of the probable effects of various possible ways of altering the situation (employment of alternative means) and the resultant choice among them. Apart from questions relating to the choice of ends and from those relating to ' effort ' . . . there is, where the standard is applicable at all, little difficulty in conceiving the actor as thus analogous to the scientist whose knowledge is the principal determinant of his action so far as his actual course conforms with the expectations of an observer who has, as Pareto says, ' a more extended knowledge of the circumstances '."

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[1] New York (McGraw Hill Book Company, Inc.) 1937, p. 58.

This definition gives an excellent résumé of the widely used concept of rational action in so far as it refers to the level of social theory. It seems important, however, to make more precise the peculiarity of this theoretical level by contrasting it with the other levels of our experience of the social world. We must, therefore, start by examining what we really mean when we speak of different levels in observing the social world. Following this a short description of the social world as it appears to the actor within this world in his every-day life will give us an opportunity of examining whether or not the category of rationality becomes determinative for his actions. Only after these preliminaries shall we examine the social world as it is given to the scientific observer; and together with it we shall have to examine the question whether the categories of interpretation used by the scientist coincide with those used by the observed actor. Anticipating our results we may say at once that with the shift from one level to another all the conceptual schemes and all the terms of interpretation must be modified.

## II

The fact that the same object has a different appearance to various observers has been illustrated by some philosophers by the example of a city which, though always the same, appears different to different persons according to their individual standpoints. I do not wish to over-work this metaphor, but it helps to make clear the difference between our view of the social world in which we naïvely live and the social world which is the object of scientific observation. The man brought up in a town will find his way in its streets by following the habits he has acquired in his daily occupations. He may not have a consistent conception of the organisation of the city, and, if he uses the underground railway to go to his office, a large part of the city may remain unknown to him. Nevertheless, he will have a proper sense of the distances between different places and of the directions in which the different points are situated relatively to whatever he regards as the centre. This centre will usually be his home, and it may be sufficient for him to know that he will find nearby an underground line or a bus leading to certain other points to bring them all within his reach. He can, therefore, say that he knows his town, and, though this knowledge is of a very incoherent kind, it is sufficient for all his practical needs.

When a stranger comes to the town, he has to learn to orientate himself in it and to know it. Nothing is self-explanatory for him and he has to ask an expert, in this case a native, to learn how to get from one point to another. He may, of course, refer to a map of the town, but even successfully to use the map he must know the meaning of the signs on the map, the exact point within the town where he stands and its correlative on the map, and at least one more point in order correctly to relate the signs on the map to the real objects in the city.

Entirely different means of orientation must be used by the cartographer who has to draw a map of the city. There are several ways open to him. He can start with a photograph taken from an aeroplane; he can place a theodolite at a known point, measure a certain distance and calculate trigonometrical functions, etc. The science of cartography has developed a standard for such operations, elements the cartographer must know before he begins to draw his map, and rules he must observe if he is to draw his map correctly.

The town is the same for all the three persons we have mentioned—the native, the foreigner and the cartographer—but for the native it has a special meaning: "my home town"; for the foreigner it is a place within which he has to live and work for some time; for the cartographer it is an object of his science, he is interested in it only for the purposes of drawing a map. We may say that the same object is considered from different levels.

We should certainly be surprised if we found a cartographer in mapping a town restricting himself to collecting information from natives. Nevertheless, social scientists frequently choose this strange method. They forget that their scientific work is done on a level of interpretation and understanding different from the naïve attitudes of orientation and interpretation peculiar to people in daily life. When these social scientists speak of different levels, they frequently consider the difference between the two levels as entirely and simply one of the degree of concreteness or generality. These two terms, however, are no more than chapter headings for much more complicated problems than those which they directly suggest.

In our daily life, as in our scientific world, we, as human beings, all have the tendency to presume, more or less naïvely, that what we have once verified as valid will remain valid throughout the future, and that what appeared to us beyond question yesterday will still be beyond all question to-morrow. This naïve presumption may be made without danger if we deal with propositions of a purely logical character, or with empirical statements of a very high generality, though it can be shown that these kinds of propositions, too, have only a limited realm of applicability. On the other hand, at a so-called concrete level, we are forced to admit very many suppositions and implications as beyond question. We can even consider the level of our actual research as defined by the total of unquestioned presuppositions which we make by placing ourselves at the specific standpoint from which we envisage the interrelation of problems and aspects under scrutiny. Accordingly, passing from one level to another would involve that certain presuppositions of our research formerly regarded as beyond all question would now be called in question ; and what was formerly a *datum* of our problem would now become problematic itself. But the simple fact that new problems and aspects of facts emerge with the shift in the point of view, while others that were formerly in the centre of our question disappear, is sufficient to initiate a thorough modification of the meaning of all the terms correctly used at the former level. Careful control of such modifications of the meaning is, therefore, indispensable if we are to avoid the risk of naïvely taking over from one level to another terms and propositions whose validity is essentially limited to a certain level, that is, to its implied suppositions.

Philosophical and in particular phenomenological theory has made very important contributions toward the better understanding of this phenomenon. However, we need not concern ourselves here with this very complicated problem from the phenomenological viewpoint. It will be sufficient to refer to an outstanding thinker of the English speaking world ; to William James and his theory of conception. It was he who taught us that each of our concepts has its fringes surrounding a nucleus of its unmodified meaning. " In all our voluntary thinking," he says, " there is some topic or subject about which all the members of the thought revolve. Relation to our topic or interest is constantly felt in the fringe of our concepts. Each word in a sentence is felt, not only as a word, but as having a meaning. The meaning of a word taken thus dynamically in a sentence may be quite different from its meaning if taken statically or without context."

It is not for us to discuss here James's theory of the nature of such fringes and their genesis in the stream of thought. For our purpose it will be sufficient to say that already the connection in which a concept or a term is used, and its relation to the topic of interest (and this topic of interest is in our case the *problem*), create specific modifications of the fringes surrounding the nucleus, or even of the nucleus itself. It was also William James who explained that we do not apperceive isolated phenomena, but rather a field of several interrelated and interwoven things as it emerges in the stream of our thought. This theory explains sufficiently for our purposes the phenomenon of the meaning of a term being modified as we pass to another level. I think that these superficial references will be sufficient to indicate the nature of the problem we are dealing with.

The term " rationality ", or at least the concept it envisages, has, within the framework of social science, the specific rôle of a " key concept ". It is peculiar to key concepts that, once introduced into an apparently uniform system, they constitute the differentiations between the points of view which we call levels. The meaning of such key concepts, therefore, does not depend on the level of the actual research, but, on the contrary, the level on which the research may be done depends upon the meaning attributed to the key concept, the introduction of which has for the first time divided what formerly appeared as a homogeneous field of research into several different levels. Anticipating what we shall have to prove later, we shall say that the level made accessible by the introduction of the term " rational action " as a chief principle of the method of social sciences is nothing else than the level of theoretical observation and interpretation of the social world.

## III

As scientific observers of the social world, we are not practically but only cognitively interested in it. That means we are not acting in it with full responsibility for the consequences, but rather contemplating it with the same detached equanimity as physicists contemplate their experiments. But let us remember that notwithstanding our scientific activity we all remain human beings in our daily life—men among fellow men with whom we are interrelated in very many ways. To be precise, even our scientific activity itself is based on the co-operation between us, the scientists, and our teachers and the teachers of our teachers, a co-operation by mutual influence and mutual criticism; but in so far as scientific activity is socially founded, it is one among other emanations of our human nature and certainly pertains to our daily life, governed by the categories of vocation and avocation, of work and leisure, of planning and accomplishing. Scientific activity as a social phenomenon is one thing, the specific attitude the scientist has to adopt toward his problem is another. Considered purely as a human activity, scientific work is distinguished from other human activities merely by the fact that it constitutes the archetype for rational interpretation and rational action.

In our daily life it is only very rarely that we act in a rational way if we understand this term in the meaning envisaged in Professor Parsons' previously quoted statement. We do not even interpret the social world surrounding us in a rational way, except under special circumstances which compel us to leave our basic attitude of just living our lives. Each of us, so it seems, has naïvely organised his social world and his daily life in such a way that he finds himself the centre of the social cosmos surrounding him. Or, better, he was already born into an organised social cosmos. For him it is a cosmos and it is organised in so far as it contains all the comfortable equipment to render his daily living and that of his fellow men a routine matter. There are, on the one hand, institutions of various kinds, tools, machines, etc.; on the other hand, habits, traditions, rules and experiences, both actual and vicarious. Furthermore, there is a scale of systematised relations which everyone has with his fellows, starting from the relations with members of his immediate family, relations with kinsmen, with personal friends, with people he knows personally, with people he met once in his life, through relations with those anonymous men who work somewhere and in a way he cannot imagine, but with the result that the letter he puts into the pillar box reaches the addressee in time, and that his lamp is lit by the turn of a switch.

Thus the social world with the "*alter ego*"s in it is arranged around the self as a centre in various degrees of intimacy and anonymity. Here am I and next to me are "*alter ego*"s of whom, as Kipling says, I know "their naked souls". Then come those with whom I share time and space and whom I know more or less intimately. Next in order are the manifold relations I have with people in whose personality I am interested, though I have only an indirect knowledge of them such, for instance, as may be obtained from their works or writings or from reports from others. For example, my social relation with the author of the book I am reading is of this kind. On the other hand, I have social relations (in the technical meaning of this term), though superficial and inconsistent ones, with others in whose personalities I am not interested, but who merely happen to perform functions in which I am interested. Perhaps the salesgirl in the store where I buy my shaving cream, or the man who polishes my shoes, are much more interesting personalities than many of my friends. I do not inquire. I am not interested in social contact with those people. I just want to get my shaving cream and to have my shoes polished by whatever means. In this sense it makes very little difference to me whether, when I want to make a telephone call, an operator or a dial intervenes. Incidentally—and here we enter the remotest sphere of social relations—the dial, too, has its social function because it derives, as do all products of human activity, from the man who invented, designed and produced it. But if I am not guided by a special motive, I do not ask for the history, genesis, and construction of all the tools and institutions created by other people's activity. Likewise, I do not ask about the personality and destiny of fellow men whose activity I consider as a purely *typical* function. In any case, and this is important for our problem, I can use the telephone with success without knowing how it functions; I am interested only in the fact that it does function. I do not care whether the result achieved, which alone interests me, is due to the intervention of a human being whose motives remain undisclosed to me or to a mechanism whose operation I do not understand. What counts is the typical character of the occurrence within a typified situation.

Thus, in this organisation of the social world by the human being living naïvely in it, we already find the germ of the system of types and typical relations which we shall recognise later in its fullest ramification as the essential feature of scientific method. This typification is progressive in the same proportion as the personality of the fellow man disappears beyond the undisclosed anonymity of his function. If we want to do so, we may interpret this process of progressive typification also as one of rationalisation. At least it is envisaged by one of the several meanings Max Weber attributes to the term " rationalisation " when he speaks of the " disenchantment of the world " (*Entzauberung der Welt*). This term means the transformation of an uncontrollable and unintelligible world into an organisation which we can understand and therefore master, and in the framework of which prediction becomes possible.

In my opinion, the fundamental problem of the different aspects under which our fellow men and their behaviour and actions seem given to us has not yet received from sociologists the attention it merits. But if social science, with few exceptions, has failed to consider this kind of rationalisation of its conceptual framework, each of us human beings, in " just living along ", has already performed this task, and this without planning to do so and without any effort in the performance of his job. In doing so, we are guided neither by methodological considerations nor by any conceptual scheme of means-end relations, nor by any idea of values we have to realise. Our practical interest alone, as it arises in a certain situation of our life, and as it will be modified by the change in the situation which is just on the point of occurring, is the only relevant principle in the building up of the perspective structure in which our social world appears to us in daily life. For, just as all our visual apperceptions are in conformity with the principles of perspective and convey the impressions of depths and distance, so all our apperceptions of the social world necessarily have the basic character of perspective views. Of course the social world of a sixty-year old Chinese Buddhist in the time of the Ming dynasty will be organised in quite a different way from the social world of a twenty-year old American Christian of our own day, but the fact remains that both worlds would be organised, and this within the framework of the categories of familiarity and strangeness, of personality and type, of intimacy and anonymity. Furthermore, each of these worlds would be centred in the self of the person who lives and acts in it.

## IV

But let us proceed in our analysis of the knowledge that a man living naïvely has about the world, the social world as well as the natural. In his daily life the healthy, adult, and wide-awake human being (we are not speaking of others) has this knowledge, so to speak, automatically at hand. From heritage and education, from the manifold influences of tradition, habits and his own previous reflection, his store of experiences is built up. It embraces the most heterogeneous kinds of knowledge in a very incoherent and confused state. Clear and distinct experiences are intermingled with vague conjectures; suppositions and prejudices cross well-proven evidences; motives, means and ends, as well as causes and effects, are strung together without clear understanding of their real connections. There are everywhere gaps, intermissions, discontinuities. Apparently there is a kind of organisation by habits, rules, and principles which we regularly apply with success. But the origin of our habits is almost beyond our control; the rules we apply are rules of thumb and their validity has never been verified. The principles we start from are partly taken over uncritically from parents and teachers, partly distilled at random from specific situations in our lives or in the lives of others without our having made any further inquiry into their consistency. Nowhere have we a guarantee of the reliability of all of these assumptions by which we are governed. On the other hand, these experiences and rules are sufficient to us for mastering life. As we normally have to act and not to reflect in order to satisfy the demands of the moment, which it is our task to master, we are not interested in the "quest for certainty". We are satisfied if we have a fair chance of realising our purposes, and this chance, so we like to think, we have if we set in motion the same mechanism of habits, rules and principles which formerly stood the test and which still stand the test. Our knowledge in daily life is not without hypotheses, inductions, and predictions, but they all have the character of the approximate and the typical. The ideal of every-day knowledge is not certainty, nor even probability in a mathematical sense, but just likelihood. Anticipations of future states of affairs are conjectures about what is to be hoped or feared, or at best, about what can be reasonably expected. When afterwards the anticipated state of affairs takes some form in actuality, we do not say that our prediction has come true or proved false, or that our hypothesis has stood the test, but that our hopes or fears were or were not well founded. The consistency of this system of knowledge is not that of natural *laws*, but that of *typical* sequences and relations.

This kind of knowledge and its organisation I should like to call "cookery-book knowledge". The cookery-book has recipes, lists of ingredients, formulæ for mixing them, and directions for finishing off. This is all we need to make an apple pie, and also all we need to deal with the routine matters of daily life. If we enjoy the apple pie so prepared, we do not ask whether the manner of preparing it as indicated by the recipe is the most appropriate from the hygienic or alimentary point of view, or whether it is the shortest, the most economical, or the most efficient. We just eat and enjoy it. Most of our daily activities from rising to going to bed are of this kind. They are performed by following recipes reduced to automatic habits or unquestioned platitudes. This kind of knowledge is concerned only with the regularity as such of events in the external world irrespective of its origin. Because of this regularity it can be reasonably expected that the sun will rise to-morrow morning. It is equally regular, and it can, therefore, with as good reason be anticipated too that the bus will bring me to my office if I choose the right one and pay my fare.

## V

The foregoing remarks characterise in a very superficial manner the conceptual scheme of our every-day behaviour in so far as the term "conceptual scheme" can be applied at all. Are we to classify a behaviour of the type just described as rational or irrational? In order to answer this question we must analyse the various equivocal implications which are hidden in the term "rationality" as it is applied to the level of every-day experience.

1. "Rational" is frequently used as synonymous with "reasonable." Now we certainly act in our daily life in a reasonable way if we use the recipes we find in the store of our experience as already tested in an analogous situation. But acting rationally often means avoiding mechanical applications of precedents, dropping the use of analogies, and searching for a new way to master the situation.

2. Sometimes rational action is put on a par with acting deliberately, but the term "deliberately" itself implies many equivocal elements.

- (*a*) Routine action of daily life is deliberated in so far as it always relates back to the original act of deliberation which once preceded the building up of the formula now taken by the actor as a standard for his actual behaviour.
- (*b*) Conveniently defined, the term "deliberation" may cover the insight into the applicability to a present situation of a recipe which has proved successful in the past.
- (*c*) We can give the term "deliberation" a meaning covering the pure anticipation of the end—and this anticipation is always the motive for the actor to set the action going.
- (*d*) On the other hand, the term "deliberation" as used, for instance, by Professor Dewey in his *Human Nature and Conduct*, means "a dramatic rehearsal in imagination of various competing possible lines of action". In this sense, which is of the greatest importance for the theory of rationality, we cannot classify as rational the type of every-day actions which we have examined up to now as deliberated actions. On the contrary, it is characteristic of these routine actions that the problem of choice between different possibilities does not enter into the consciousness of the actor. We shall have to come back to the problem of choice immediately.

(3) Rational action is frequently defined as "planned" or "projected" action without a precise indication of the meaning of the terms "planned" or "projected". We cannot simply say that the non-rational routine acts of daily life are not consciously planned. On the contrary, they rest within the framework of our plans and projects. They are even instruments for realising them. All planning

presupposes an end to be realised by stages, and each of these stages may be called, from one point of view or another, either means or intermediate ends. Now the function of all routine work is a standardisation and mechanisation of the means-end relations as such by referring standardised means to standardised classes of ends. The effect of this standardisation is that the intermediate ends disappear from the consciously envisaged chain of means which have to be brought about for performing the planned end. But here arises the problem of subjective meaning which we have mentioned before. We cannot speak of the unit-act as if this unit were constituted or demarcated by the observer. We must seriously ask: when does one act start and when is it accomplished? We shall see that only the actor is in a position to answer this question.

Let us take the following example: Assume the professional life of a business man to be organised and planned to the extent that he intends to continue with his business for the next ten years, after which he hopes to retire. To continue his work involves going to his office every morning. For this purpose he has to leave his home at a certain hour, buy a ticket, take the train, etc. He did so yesterday and he will do so to-morrow if nothing extraordinary intervenes. Let us assume that one day he is late and that he thinks: " I shall miss my train—I shall be late at my office. Mr. 'X' will be there already, waiting for me. He will be in a bad humour, and perhaps he will not sign the contract on which so much of my future depends." Let us further assume that an observer watches this man rushing for the train " as usual " (as he thinks). Is his behaviour planned, and if so, what is the plan? Only the actor can give the answer because he alone knows the span of his plans and projects. Probably all routine work is a tool for bringing about ends which are beyond routine work and which determine it.

4. " Rational " is frequently identified with " predictable ". It is not necessary to return to this question. We have already analysed the specific form of prediction in every-day knowledge as simply an estimate of likelihood.

5. According to the interpretation of some authors, " rational " refers to " logical." Professor Parsons' definition is one example and Pareto's theory of non-logical action to which he refers is another. In so far as the scientific concept of the rational act is in question, the system of logic may be fully applied. On the other hand, on the level of every-day experience, logic in its traditional form cannot render the services we need and expect. Traditional logic is a logic of concepts based on certain idealisations. In enforcing the postulate of clearness and distinctness of the concepts, for instance, traditional logic disregards all the fringes surrounding the nucleus within the stream of thought. On the other hand, thought in daily life has its chief interest precisely in the relation of the fringes which attach the nucleus to the actual situation of the thinker. This is clearly a very

important point. It explains why Husserl classifies the greater part of our propositions in daily thought as "occasional propositions", that means, as valid and understandable only relative to the speaker's situation and to their place in his stream of thought. It explains, too, why our every-day thoughts are less interested in the antithesis "true–false" than in the sliding transition "likely–unlikely". We do not make every-day propositions with the purpose of achieving a formal validity within a certain realm which could be recognised by someone else, as the logician does, but in order to gain knowledge valid only for ourselves and to further our practical aims. To this extent, but only to this extent, the principle of pragmatism is incontestably well founded. It is a description of the style of every-day thought, but not a theory of cognition.

6. A rational act presupposes, according to the interpretation of other authors, a choice between two or more means toward the same end, or even between two different ends, and a selection of the most appropriate. This interpretation will be analysed in the following section.

## VI

As Professor John Dewey has pointed out, in our daily life we are largely preoccupied with the next step. Men stop and think only when the sequence of doing is interrupted, and the disjunction in the form of a problem forces them to stop and rehearse alternative ways—over, around or through—which their past experience in collision with this problem suggest. The image of a dramatic rehearsal of future action used by Professor Dewey is a very fortunate one. Indeed, we cannot find out which of the alternatives will lead to the desired end without imagining this act as already accomplished. So we have to place ourselves mentally in a future state of affairs which we consider as already realised, though to realise it would be the end of our contemplated action. Only by considering the act as accomplished can we judge whether the contemplated means of bringing it about are appropriate or not, or whether the end to be realised accommodates itself to the general plan of our life. I like to call this technique of deliberation "thinking in the future perfect tense". But there is a great difference between action actually performed and action only imagined as performed. The really accomplished act is irrevocable and the consequences must be borne whether it has been successful or not. Imagination is always revocable and can be revised again and again. Therefore, in simply rehearsing several projects, I can ascribe to each a different probability of success, but I can never be disappointed by its failure. Like all other anticipations, the rehearsed future action also has gaps which only the performance of the act will fill in. Therefore the actor will only retrospectively see whether his project has stood the test or proved a failure.

The technique of the choice is this: The mind of the actor runs through one alternative and then through the other till the decision falls from his mind—to use the words of Bergson—as a ripe fruit falls from the tree. But it is a pre-requisite of all choice that the actor have clearly in mind that alternative ways of applying different means or even alternative ends do actually exist. It is erroneous to assume that consciousness of such alternatives and therefore choice is necessarily given before every human action, and that in consequence all acting involves deliberation and preference. This interpretation uncritically confuses selection in the sense of just singling out without comparison of alternatives, and choice in the sense of electing the preferred. Selection is, as has already been pointed out by James, a cardinal function of human consciousness. Interest is nothing else than selection, but it does not necessarily involve conscious choice between alternatives which presupposes reflection, volition, and preference. When I walk through a garden discussing a problem with a friend and I turn left or right, I do not choose to do so. I have no alternative in mind. It is a question for psychology to determine the motives for such behaviour, but I cannot say that I prefer one direction to another.

Undoubtedly there are situations in which each of us sits down and thinks over his problems. In general he will do so at critical points in his life when his chief interest is to master a situation. But even then he will accept his emotions as guides in finding the most suitable solution as well as rational deliberation, and he is right in doing so, because these emotions also have their roots in his practical interest.

He will also appeal to his stock of recipes, to the rules and skill arising out of his vocational life or his practical experiences. He will certainly find many systematised solutions in his standardised knowledge. He may perhaps consult an expert, but again he will get nothing else than recipes and systematised solutions. His choice will be a deliberated one, and having rehearsed all the possibilities of action open to him in the future perfect tense, he will put in action that solution which seems to have the greatest chance of success.

But what are the conditions under which we may classify a deliberated act of choice as a rational one? It seems that we have to distinguish between the rationality of knowledge which is a prerequisite of the rational choice and the rationality of the choice itself. Rationality of knowledge is given only if all the elements from which the actor has to choose are clearly and distinctly conceived by him. The choice itself is rational if the actor selects from among all means within his reach the one most appropriate for realising the intended end.

We have seen that clearness and distinctness in the strict meaning of formal logic do not belong to the typical style of every-day thought. But it would be erroneous to conclude that, therefore, rational choice does not exist within the sphere of every-day life. Indeed, it would be sufficient to interpret the terms clearness and distinctness in a modified and restricted meaning, namely, as clearness and distinctness adequate to the requirements of the actor's practical interest. It is not our task to examine whether rational acts corresponding with the above-mentioned characteristics do or do not occur frequently in daily life. There is no doubt that " rational acts " together with their antithesis, defined by Max Weber as " traditional " or " habitual " acts, represent rather ideal types which will be found very seldom in their pure form in every-day action. What I wish to emphasise is only that the ideal of rationality is not and cannot be a peculiar feature of every-day thought, nor can it, therefore, be a methodological principle of the interpretation of human acts in daily life. This will become clearer if we discuss the concealed implications of the statement—or better, postulate—that rational choice would be present only if the actor had sufficient knowledge of the end to be realised as well as of the different means apt to succeed. This postulate implies :

(*a*) Knowledge of the place of the end to be realised within the framework of the plans of the actor (which must be known by him, too).

(*b*) Knowledge of its interrelations with other ends and its compatibility or incompatibility with them.

(*c*) Knowledge of the desirable and undesirable consequences which may arise as by-products of the realisation of the main end.

(*d*) Knowledge of the different chains of means which technically or even ontologically are suitable for the accomplishment of this end, regardless of whether the actor has control of all or several of their elements.

(*e*) Knowledge of the interference of such means with other ends or other chains of means including all their secondary effects and incidental consequences.

(*f*) Knowledge of the accessibility of those means for the actor, picking out the means which are within his reach and which he can and may set going.

The aforementioned points do not by any means exhaust the complicated analysis which would be necessary in order to break down the concept of rational choice in action. The complications increase greatly when the action in question is a social one, that is, when it is directed towards other people. In this case, the following elements become additional determinants for the deliberation of the actor :

First : The interpretation or misinterpretation of his own act by his fellow man.

Second : The reaction by the other people and its motivation.

Third : All the outlined elements of knowledge (*a* to *f*) which the actor, rightly or wrongly, attributes to his partners.

Fourth : All the categories of familiarity and strangeness, of intimacy and anonymity, of personality and type, which we have discovered in the course of our inventory of the organisation of the social world.

This short analysis shows that we cannot speak of an *isolated* rational act, if we mean by this an act resulting from deliberated choice, but only of a *system* of rational acts.[1]

But where is this *system* of rational action to be found ? We have already noted that the concept of rationality has its native place not at the level of the every-day conception of the social world, but at the theoretical level of the scientific observation of it, and it is here that it finds its field of methodological application. Therefore, we have to proceed to the problem of the social sciences and to the scientific methods of its interpretation.

## VII

Our analysis of the social world in which we live has shown that each of us considers himself as the centre of this world, which he groups around himself according to his own interests. The observer's attitude towards the social world is quite different. This world is not the theatre of his activities, but the object of his contemplation on which he looks with detached equanimity. As a scientist (not as a human being dealing with science) the observer is essentially solitary. He has no companion and we can say that he has placed himself outside the social world with its manifold relations and its system of interests. Everyone, to become a social scientist, must make up his mind to put somebody else instead of himself as the centre of this world, namely, the observed person. But with the shift in the central point, the whole system has been transformed, and, if I may use this metaphor, all the equations proved as valid in the former system now have to be expressed in terms of the new one. If the social system in question had reached an ideal perfection, it would be possible to establish a universal transformation formula such as Einstein has succeeded in establishing for translating propositions in terms of the Newtonian System of Mechanics into those of the theory of Relativity.

The first and fundamental consequence of this shift in the point of view is that the scientist replaces the human beings he observes as actors on the social stage by puppets created by himself and manipulated by himself. What I call " puppets " corresponds to the technical term " ideal types " which Weber has introduced into Social Science.

Our analysis of our common social world has shown us the origin of typification. We typify, in daily life, human activities which interest us only as appropriate means for bringing about intended effects, but not as emanations of the personality of our fellow men.

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[1] See the excellent study which Professor Parsons has devoted to this problem under the heading " Systems of Action and their Unit," at the end of his *Structure of Social Action.*

The procedure of the scientific observer is on the whole the same. He observes certain events as caused by human activity and he begins to establish a type of such proceedings. Afterwards he co-ordinates with these typical acts typical actors as their performers. He thus ends up by constructing personal ideal types which he imagines as having consciousness. This fictitious consciousness is constructed in such a way that the fictitious actor, if he were not a dummy but a human being of flesh-and-blood, would have the same stream of thought as a living man acting in the same manner, but with the important modification that the artificial consciousness is not subjected to the ontological conditions of human existence. The puppet is not born, he does not grow up, and he will not die. He has no hopes and no fears ; he does not know anxiety as a chief motive of all his deeds. He is not free in the sense that his acting could transgress the limits his creator, the social scientist, has fixed. He cannot, therefore, have other conflicts of interest and motives than those the social scientist has implanted in him. The personal ideal type cannot err if to err is not its typical destiny. It cannot perform an act which is outside the typical motives, the typical means-ends relations, and outside the typical situation provided for by the scientist. In short, the ideal type is but a model of a conscious mind without the faculty of spontaneity and without a will of its own. In typical situations of our daily life we all, too, assume certain typical rôles. By isolating one of our activities from its interrelations with all the other manifestations of our personality, we disguise ourselves as consumers or tax-payers, citizens, members of a church or of a club, clients, smokers, by-standers, etc. The traveller, for instance, has to behave in the specific way he believes the type " railway agent " to expect from a typical passenger. For us in our daily lives these attitudes are but rôles which we voluntarily assume as expedients and which we may drop whenever we want to do so. But assuming this rôle does not change our general attitude toward the social world or toward our own life. Our knowledge remains incoherent, our propositions occasional, our future uncertain, our general situation unstable. The next moment may bring the great cataclysm which will affect our choice, modify all our plans, perhaps destroy the value of all our experience. And we keep—even in the rôle—the liberty of choice, as far as such liberty exists at all within the scope of our human and social conditions. This liberty embraces the possibility of taking off our disguise, of dropping the rôle, of recommencing our orientation in the social world. We continue to be subjects, centres of spontaneous activities, actors.

The puppet called " personal ideal type " is, on the contrary, never a subject or a centre of spontaneous activity. He does not have the task of mastering the world, and, strictly speaking, he has no world at all. His destiny is regulated and determined beforehand by his creator, the social scientist, and in such a perfect pre-established harmony as Leibnitz imagined the world created by God. By the grace of its constructor, he is endowed with just that kind of knowledge he needs to perform the job for the sake of which he was brought into the scientific world. The scientist distributes his own store of experience, and that means of scientific experience in clear and distinct terms, among the puppets with which he peoples the social world. But this social world, too, is organised in quite another way; it is not centred in the ideal type; it lacks the categories of intimacy and anonymity, of familiarity and strangeness: in short, it lacks the basic character of perspective appearance. What counts is the point of view from which the *scientist* envisages the social world. This point of view defines the general perspective framework in which the chosen sector of the social world presents itself to the scientific observer as well as to the fictitious consciousness of the puppet type. This central point of view of the scientist is called his "scientific problem under examination."

In a scientific system the problem has exactly the same significance for the scientific activity as the practical interests have for activities in every-day work. The scientific problem as formulated has a two-fold function:

(*a*) It determines the limits within which possible propositions become relevant to the inquiry. It thus creates the realm of the scientific subject matter within which all concepts must be compatible.

(*b*) The simple fact that a problem is raised creates a scheme of reference for the construction of all ideal types which may be utilised as relevant.

For the better understanding of the last remark we have to consider that the concept "type" is not an independent one but always needs a supplement. We cannot speak simply of an "ideal type" as such; we must indicate the reference scheme within which this ideal type may be utilised, that is, the problem for the sake of which the type has been constructed. To borrow a mathematical term we may say that the ideal type always needs a subscript referring to the problem which determines the formation of all the types to be used. In this sense, the problem under scrutiny is the locus of all possible types which may pertain to the system investigated.

I cannot go further here into the logical foundations of this thesis which I call the *principle of relevance*. But we can interpret it as an application of James's theory concerning the fringes of concepts. The ideal type, too, like all other concepts, has fringes referring to the main topic about which all the members of the thought revolve. It is easy to understand that a shift in the main topic—that is, in the problem—automatically involves a modification in the fringes of each concept revolving about it. And, as a shift in the problem means a modification in the scope of relevance too, we can explain, for the same reason, why new facts emerge with the shift in the point of view, whereas others that were formerly in the centre of our question disappear. But this statement is nothing else than the original definition we have given of the passing from one level to another. To be sure, it must be admitted that the term " level " applies strictly only to whole systems of problems; nevertheless, the consequences are, in principle, the same. It seems important to me that the scientist keep in mind that each shift in the problem involves a thorough modification of all the concepts and of all the types with which he is dealing. A great many misunderstandings and controversies in the social sciences have their root in the unmodified application of concepts and types at a level other than that where they have their natural place.

But why form personal ideal types at all? Why not simply collect empirical facts? Or, if the technique of typological interpretation may be applied successfully, why not restrict oneself to forming types of impersonal events, or types of the behaviour of groups? Do we not have modern economics as an example of a social science which does not deal with personal ideal types, but with curves, with mathematical functions, with the movement of prices, or with such institutions as bank systems or currency? Statistics has performed the great work of collecting information about the behaviour of groups. Why go back to the scheme of social action and to the individual actor?

The answer is this: It is true that a very great part of social science can be performed and has been performed at a level which legitimately abstracts from all that happens in the individual actor. But this operating with generalisations and idealisations on a high level of abstraction is in any case nothing but a kind of intellectual shorthand. Whenever the problem under inquiry makes it necessary, the social scientist must have the possibility of shifting the level of his research to that of individual human activity, and where real scientific work is done this shift will always become possible.

The real reason for this is that we cannot deal with phenomena in the social world as we do with phenomena belonging to the natural sphere. In the latter, we collect facts and regularities which are not understandable to us, but which we can refer only to certain fundamental assumptions about the world. We shall never understand why the mercury in the thermometer rises if the sun shines on it. We can only interpret this phenomenon as compatible with the laws we have deduced from some basic assumption about the physical world. Social phenomena, on the contrary, we want to understand and we cannot understand them otherwise than within the scheme of human motives, human means and ends, human planning—in short—within the categories of human action.

The social scientist must therefore ask, or he must, at least, always be in a position to ask, what happens in the mind of an individual actor whose act has led to the phenomenon in question. We can formulate this *postulate of the subjective interpretation* more correctly as follows: The scientist has to ask what type of individual mind can be constructed and what typical thoughts must be attributed to it to explain the fact in question as the result of its activity within an understandable relation.

This postulate finds its complement in another which I propose to call, borrowing a term of Max Weber, the *postulate of adequacy*. It may be formulated as follows: " Each term used in a scientific system referring to human action must be so constructed that a human act performed within the life world by an individual actor in the way indicated by the typical construction would be reasonable and understandable for the actor himself, as well as for his fellow men." This postulate is of extreme importance for the methodology of social science. What makes it possible for a social science to refer at all to events in the life world is the fact that the interpretation of any human act by the social scientist might be the same as that by the actor or by his partner.

The principle of relevance, the postulate of the subjective interpretation, and that of adequacy, are applicable at each level of social study. For instance, all the historical sciences are governed by them. The next step would be to circumscribe within the social sciences the category of those we call the theoretical ones. The outstanding feature of these theoretical sciences is the interpretation of the social world in terms of a system of determinate logical structure (Parsons, *op. cit.*, p. 7). This system of means-ends relations is also an ideal typical one, but as Professor Parsons has pointed out, it is an analytical one and not a system dealing with concrete actions as he calls it. I once formulated the same idea in the statement that the personal ideal-types of action constructed by the so-called theoretical sciences are of a maximum anonymity, that means, what is typified is the behaviour of " people as such " or of " every-man ". Whatever formula we may use to describe the peculiarity of the theoretical realm, it is clear that a logically interrelated system presupposes that the means-end relations together with the system of constant motives and the system of life plans must be constructed in such a way that:

(*a*) it remains in full compatibility with the principles of formal logic;

(*b*) all its elements are conceived in full clearness and distinctness;

(*c*) it contains only scientifically verifiable assumptions which have to be in full compatibility with the whole of our scientific knowledge.

All these three requirements may be condensed into another *postulate* for the building up of the ideal types, that of *rationality*. It may be formulated as follows: The ideal type of social action must be constructed in such a way that the actor in the living world would perform the typified act if he had a clear and distinct scientific knowledge of all the elements relevant to his choice and the constant tendency to choose the most appropriate means for the realisation of the most appropriate end. Indeed, as we had anticipated in the beginning, only by the introduction of the key concept of rationality can all the elements be provided for the constitution of the level called "pure theory". The postulate of rationality implies, furthermore, that all other behaviour has to be interpreted as derivative from the basic scheme of rational acting. The reason for this is that only action within the framework of rational categories can be scientifically discussed. Science does not have at its disposal other methods than rational ones and it cannot, therefore, verify or falsify purely occasional propositions.

As we stated before, each type formed by the scientist has its subscript referring to the main problem. In a theoretical system, therefore, only pure rational types are admitted. But where can the scientist find the guarantee that he is establishing a real unified system? Where are the scientific tools to perform that difficult task? The answer is that in every branch of the social sciences which has arrived at the theoretical stage of its development there is a fundamental hypothesis which both defines the fields of research and gives the regulative principle for building up the system of ideal types. Such a fundamental hypothesis, for instance, is in classical economics the utilitarian principle, and in modern economics the marginal principle. The sense of this postulate is the following: "Build your ideal types as if all actors had oriented their life plan and, therefore, all their activities to the chief end of realising the greatest utility with the minimum of costs; human activity which is oriented in such a way (and only this kind of human activity) is the subject matter of your science."

But at the back of all these statements arises a very disturbing question. If the social world as object of our scientific research is but a typical construction, why bother with this intellectual game? Our scientific activity and, particularly, that which deals with the social world, is also performed within a certain means-ends relation, namely, in order to acquire knowledge for mastering the world, the real world, not the one created by the grace of the scientist. We want to find out what happens in the real world and not in the fantasies of a few sophisticated eccentrics.

There are a few arguments for quieting such an interlocutor. First of all, the construction of the scientific world is not an arbitrary act of the scientist which he can perform at his own discretion:

1. There are the historical boundaries of the realm of his science which each scientist has inherited from his ancestors as a stock of approved propositions.

2. The postulate of adequacy requires that the typical construction be compatible with the totality of both our daily life and our scientific experience.

But to one who is not satisfied with such guarantees and asks for greater reality, I want to say that I am afraid I do not exactly know what reality is, and my only comfort in this unpleasant situation is that I share my ignorance with the greatest philosophers of all time. Again, I want to quote William James and his profound theory of the different realities in which we live at the same time. It is a misunderstanding of the essential character of science to think that it deals with reality if we consider as the pattern of reality the world of daily life. The world of both the natural and the social scientist is neither more nor less real than the world of thought in general can be. It is not the world within which we act and within which we are born and die. But it is the real home of those important events and achievements which humanity at all times calls culture.

The social scientist, therefore, may continue his work in full confidence. His clarified methods, governed by the postulates mentioned, give him the assurance that he will never lose contact with the world of daily life. And as long as he uses with success methods which have stood this test and still do so, he is quite right in continuing without worrying about methodological problems. Methodology is not the preceptor or the tutor of the scientist. It is always his pupil, and there is no great master in his scientific field who could not teach the methodologists how to proceed. But the really great teacher always has to learn from his pupils. Arnold Schoenberg, the famous composer, starts the preface to his masterly book on the theory of harmony with the sentence: "This book I have learned from my pupils." In this rôle, the methodologist has to ask intelligent questions about the technique of his teacher. And if those questions help others to think over what they really do, and perhaps to eliminate certain intrinsic difficulties hidden in the foundation of the scientific edifice where the scientists never set foot, methodology has performed its task.

# Multilateral Clearing[1]

By E. F. Schumacher

Let us assume for a start that there are certain advantages to be gained from settling international payments for goods and services by way of Clearing. The importer in country *A* pays for the goods he buys from country *B* by handing over to the Clearing Authority in his own country a sum of *A*-money which is deemed to discharge his debt. The exporter in country *B* receives from the Clearing Authority in his country an equivalent sum of *B*-money which is deemed to satisfy his claim.

There is little difficulty in organising a Clearing of this sort to cover the trade between any two countries. Not only payments for goods and services, patents and copyrights, but also dividend, interest and amortisation payments can be settled over Clearing. These "bilateral clearings" exert a strong tendency, unless counteracting measures are taken, to equalise the trade between the two countries concerned by either stimulating the exports of the country which would otherwise have had an import surplus, or curtailing the exports of the country which would otherwise have had an export surplus.

Let us now assume that a system of trading which enforces strict Bilateralism has certain definite disadvantages and that it would be desirable to maintain the multilateral character of trade, whereby no country finds itself under the necessity to balance its trade with any one particular other country, but can feel free to buy even from countries which are not its customers and to sell even to countries from which it does not buy.

The question then arises: Can we have multilateral clearing?

The central problem of multilateral clearing can be stated as follows: Assume that there are only three countries, say, the United States, Britain and Poland. And assume that in the course of one year's trading America sells more than she buys, Britain sells just as much as she buys, and Poland buys more than she sells. America could be called a "surplus country", Poland a "deficit country", and Britain a country which has achieved "balance". These designations refer to the *total* trade of each country; the bilateral trade may be quite different. Polish-American trade, for instance, may come out even, which would mean (on our assumptions) that the trade between Poland and Britain showed a Polish deficit, and the trade between Britain and America a British deficit. If that is the case, America will have uncleared sterling balances in Britain, and Britain will have uncleared zloty-balances in Poland. Multilateral clearing implies that in this case Britain, having achieved (global) balance, should be " out of it ", in the sense that she is under no obligation either to curtail her imports from America, or to press her exports to America, or to curtail her exports to Poland, or to increase her imports from Poland. But Britain is released from these obligations only if the Americans are prepared to exchange their sterling-balances for Britain's zloty-balances. Why should they be prepared to do this ? Sterling balances (on our assumptions) are preferable to zloty-balances, because Britain, at least, is a country which has achieved " balance " in her total trade, while Poland is a deficit country. Why should the Americans exchange a better currency for one that is worse ? If they can be made to do so, then we can have multilateral clearing ; but if not, then the clearings will be strictly bilateral, and even the setting up of an International Clearing Office would not help.

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[1] This study was carried out for the Joint Committee of the Nuffield College Reconstruction Survey and the Institute of Statistics, Oxford. It has been in private circulation since November, 1942. An earlier version, entitled *Free Access to Trade*, was circulated by the Royal Institute of International Affairs in March, 1942.

In this connection it is interesting to note that some form of multilateral clearing exists at present on the continent of Europe. How does it work ? The answer is simple : Germany, as the supreme military power on the continent, forces the surplus countries to agree to the necessary exchanges of uncleared balances so that in the end the countries which have achieved balance (or : to the extent that they have achieved balance) are relieved of all claims and liabilities arising out of the different bilateral clearings, and the surplus countries simply remain as the creditors of the deficit countries.

The problem we have to face, therefore, reduces itself to the question : How can multilateral clearing be achieved without the application of force ?

The solution here offered might be called " Pool Clearing ". It works as follows :—

Every country sets up an independent agency called the National Clearing Fund. The various National Clearing Funds agree among themselves as to the rates of exchange by which each currency is to be related to all other currencies.

Importers (of goods and services) make all payments in their own national currency into their own National Clearing Fund. As soon as the Fund has received payment, it informs the National Clearing Fund of the exporter that payment has been received, whereupon the National Clearing Fund in the exporter's country makes payment to the exporter. Each National Clearing Fund thus receives and disburses only national currency : it receives such currency from the home importers and disburses it to the home exporters.[1]

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[1] The reader will appreciate that this paragraph merely states the *theoretical* principle of Pool Clearing. In practice, the canalising of all payments through the National Clearing

Assume now that dealings on this basis are carried on for a period of, say, twelve months. At the end of the period we should find that the different National Clearing Funds can be divided into three classes:

(1) The Clearing Funds of countries which have imported more than they have exported ("deficit countries") will have received more national currency from importers than they have disbursed to exporters and will consequently be left with a balance of cash in hand.

(2) The Clearing Funds of countries which have exported more than they have imported ("surplus countries") will have disbursed more national currency to their exporters than they have received from their importers and will consequently be left with a debit balance.

(3) The Clearing Funds of countries which have exported just as much as they have imported will have disbursed to their exporters just as much national currency as they have received from their importers, and will consequently be left neither with a balance of cash in hand nor with a debit balance of any kind; they will be in the same position as they were twelve months previously.

Thus the Clearing Funds of deficit countries would be left with a balance of cash in hand, and since it would be undesirable if the Clearing Funds interfered in internal monetary policy, the proper way to employ this balance would be for them to purchase Treasury bills in the open market; similarly the Clearing Funds of surplus countries would be in need of cash and should raise such cash by selling Treasury bills in the open market. In this way National Clearing Funds would operate in a manner analogous to that of Exchange Equalisation Funds.

What, then, would be the position of ownership with regard to the various balances remaining in the deficit countries? Let us revert to our example. America sells additional 10*x* to Britain, and Britain sells additional 10*x* to Poland. Britain having achieved balance is no longer concerned in the matter; her national Clearing Fund has received an extra 10*x* from importers and disbursed an extra 10*x* to exporters. It follows that America owns a balance of 10*x* in Poland, although she may never have been in trading relations with that country at all. A sterling-balance, which the Americans could own, simply does not exist. Thus the question of ownership is quite clear: the surplus country owns the cash balance in the Clearing Fund of the deficit country.

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Fund can be effected in a way hardly noticeable to any individual trader. The established banks would be authorised to make and accept payments against documentary evidence and would merely settle the uncleared balance with the National Clearing Fund. They would also continue to finance home importers and exporters in much the same way as before. Whether or not certain categories of in-payments or out-payments are to be made subject to special permit, is a question which every national government has to decide in the light of its general economic policy. Any particular degree of foreign exchange control is equally compatible with the system of Pool Clearing.

But what if there are more than three countries? Assume that America sells additional $10x$ to Britain, and Britain $8x$ to Poland and $4x$ to Holland. America has a surplus of $10x$, Britain a surplus of $2x$. Cash balances of $8x$ and $4x$ are held in the Clearing Funds of Poland and Holland respectively. To whom do they belong? What proportion of the zloty-balances and of the guilder-balances belongs to America and what to Britain? Not unless the time sequence of all individual transactions were meticulously studied and compared could this question be answered. The balances in the deficit countries have lost their identity. The innumerable threads of business cannot be disentangled. All that can be said, and all that should be said, is that 5/6ths of the total of balances in Poland and Holland belong to America and 1/6th to Britain. This is the decisive feature of "Pool Clearing".

It will be observed that the pooling of balances arises automatically out of the mechanism of the system. No surplus country is ever called upon to exchange one balance for another. Under a system of bilateral clearings, America—in our example—would get into the possession of sterling-balances even if Britain had achieved balance or even if Britain were a surplus country herself. To multilateralise such a system, America would have to be forced against her immediate interest to swap her sterling balances against Britain's balances in the deficit countries. There would be no country strong enough to enforce such swapping on a world-wide scale. Under Pool Clearing this whole question never comes up. Any such cash balances as appear in the various National Clearing Funds are always and inevitably the result of a deficit in the *total* trade of the countries in question. Mere bilateral trade deficits do not give rise to the appearance of uncleared balances, unless they represent at the same time deficits in total trade.

While the pooling of balances, however, arises automatically under the system here discussed, it will still be necessary to create some international machinery to give to this process its appropriate legal form. Let us assume, therefore, that an "International Clearing Office" is set up to act, so to speak, as Trustee in the pooling of uncleared balances. All cash balances accumulating (in the form of a holding of Treasury Bills) in the Clearing Funds of the deficit countries are deemed, at any time, to be taken over by the International Clearing Office, and the Clearing Funds of the surplus countries are deemed to own each a share in the Pool, equal to the size of their respective surpluses.

It will be clear that the International Clearing Office requires no finance of its own, nor does it have to create a new international currency.[1] Since it is impossible to disentangle the mass of individual transactions which give rise, during the course of business, to the various uncleared balances in the deficit countries and to ascribe any one particular balance, or part of it, to any one particular surplus country, the Gordian knot is cut by making all the surplus countries the joint owners of the balances in all the deficit countries. In this way, one might say, every national currency is made into a world currency, whereby the creation of a new world currency becomes unnecessary. Nor does the International Clearing Office—in *this* connection—require any special powers; it is not an agency for control, but a purely administrative body, the central accounting office for the different National Clearing Funds.

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[1] If a new international currency were created, this would be of no economic significance. The holding of "a share in the Pool" would then be called a holding of "world currency", but the backing of the world currency would none the less be nothing else but the cash balances in the deficit countries.

As a result of its (purely formal) operations, we get the following position: The Clearing Funds of surplus countries become indebted to their internal money markets and acquire an equivalent share in the Pool; both their debt and their share in the Pool being equal to their trade surplus. The Clearing Funds of the deficit countries are left with balances of cash in hand (equal to their trade deficits) which belong to the International Pool. The Clearing Funds, finally, of countries whose balance of trade has left neither surplus nor deficit hold neither cash nor a share in the Pool.

This system, obviously, is fully multilateral. It is immaterial to each individual nation where it buys or sells. Whether it sells to a deficit country or to a surplus country, its National Clearing Fund will disburse national currency to the exporters at home and thereby decrease the amount of cash held by the Fund or increase the debt owed by the Fund to the internal money market. Whether it buys from a deficit country or from a surplus country, its National Clearing Fund will receive national currency from the importers at home and thereby increase its cash holding or reduce its debt to the internal money market. All individual sales and purchases have the same technical effect, irrespective of the country to which goods are sold or from which goods are purchased.

They have the same *technical* effect. That is to say: there is nothing in the technical set-up of this system to induce any individual importer or exporter to choose his sources of supply or his markets with reference to his country's bilateral trade balances. Under a regime of a multitude of bilateral clearings this would be quite different: each country would always have some clearings with a trade surplus and some with a deficit. Britain, in our example above, would have ready cash for additional imports from Poland, but not for additional imports from America; America would have ready cash for additional imports from Britain, but not for additional purchases in Poland. Under Pool Clearing these differential stimuli do not come into existence. The identity of ownership of the various balances accumulating in deficit countries (and in deficit countries *only*) cannot be separately established. And the main stimulus that remains is for the surplus country to spend its surplus—*anywhere* in the world.

This is a great advantage, because it avoids the dangers and frustration of Bilateralism and allows world trade to flow according to whatever economic criteria may exist for the international division of labour, instead of setting up the arbitrary criterion of bilateral balance.

Yet while a system of trading that enforces strict bilateralism must be considered arbitrary and discriminatory, the same does not necessarily apply to a system that enforces balance in the *total* trade of each country. The principal aim, in fact, of any new system should be that the achievement of such global balance be facilitated. In this way all those problems might be solved which arise when one country achieves a surplus for a prolonged period of time and, being unwilling to increase its purchases, drains other countries of all their liquid means for making international payments (e.g. gold and foreign exchange balances) and ultimately may even force them into default. But balance, even though it solves some problems, is not an end in itself. The task is to achieve balance at the right level—and that level should, as far as possible, be determined by those countries that are in need of, and willing to purchase, foreign goods. This is the only possible meaning that can be attached to the " Free Access " clause in the Atlantic Charter, which promises " enjoyment by all States, great or small, victor or vanquished, of access, on equal terms, to the trade and to the raw materials of the world which are needed for their economic prosperity ". Under a regime of bilateral clearings each country has, one might say, free access to the trade and raw materials, not of the world as a whole, but only of those other countries which are its customers. Under Pool Clearing access to trade is universally free.

Yet, no matter what is the technical set-up, every country must ultimately pay for what it buys, which means, in the long run, that it must achieve a position in which it can supply as much in goods and services to the rest of the world as it receives. This does not deny the possibility of making international gifts, grants-in-aid, Lend-Lease, or reparation payments. But they fall outside the scope of our investigation, which is concerned exclusively with trade, carried on in the pursuance of gainful economic exchange. A system of multilateral clearing may be superior to a system comprising a multitude of bilateral clearings ; but both are merely technical systems, and both are dependent for their success upon more fundamental factors. The maximisation of economic benefits to be derived from international trade can be facilitated but not automatically assured by any purely technical system.

How, then, would multilateral clearing—Pool Clearing—tend to affect those more fundamental real factors upon which so much depends ?

Let us say we make a start with this system and give every country the right to discharge all its cash obligations to the rest of the world simply by paying its own national currency into its National Clearing Fund. Every Clearing Fund would be entitled to receive any payment that arises out of international economic exchange, i.e. payments for foreign goods and services of all kinds, interest, dividend and amortisation payments on old and new foreign debts, etc. Only new capital movements would have to be dealt with specially, whether they arise out of commercial lending or a private flight of capital. They will be discussed later.

Whenever a National Clearing Fund receives payment from a resident at home who wishes to discharge a debt abroad, it notifies the National Clearing Fund in the payee's country, whereupon the latter makes payment to the payee. In this way, one might say, each country gives to each other country an overdraft facility for purposes of foreign payments. This might be a good way of getting world trade started again after the war when most countries will find themselves without any international means of payment. But it cannot be expected that any country would wish to give to the rest of the world an *unlimited* overdraft upon its own resources. How should these facilities be quantitatively determined ?

There are two possibilities : each country might decide that it is prepared to allow its National Clearing Fund to run into debt with the internal money market up to a specified amount. After that amount has been reached, exporters would receive payment only to the extent that means of payment have been made available by importers. Or one might approach the problem from the other side : not from the angle of the granter of the overdraft, but from that of the recipient. Each country would be given a certain maximum limit beyond which the cash balance in its National Clearing Fund would not be allowed to rise. After that amount had been reached, the Clearing Funds in the rest of the world would take no further notice of notifications coming from the Clearing Fund of the " overdrawn " deficit country and refuse to make any further payments to their home exporters for goods delivered to that country.

These two methods of limitation come to the same in that they lead to a stoppage (or rationing) of deliveries to the overdrawn country. But their effect upon the rest of the world is quite different. Assume that there are six countries and each agrees to allow the five others jointly an overdraft not exceeding $10x$. If all countries but one then have a surplus of trade, the remaining one can go on accumulating deficits up to a total of $50x$, which may be grossly excessive. But if these six countries agree amongst themselves that no single country should be allowed to have a trade deficit exceeding $10x$, then the worst that might happen is that five countries become deficit countries each using its overdraft facilities to the full, and one country remains as the sole creditor on Pool Clearing account to the tune of $50x$.

Which of the two methods to choose involves a question of principle.

The first method of limitation makes it quite clear to each participant country what is its maximum stake in the Pool. The second method makes it quite clear to each participant what is its maximum indebtedness to the Pool. It should not be difficult to make the choice between them. The second method is the only workable one. If the first method were chosen, the maximum overdraft facility each country is to give would have to be kept so low that no one country, no matter how small, could ever become excessively indebted. This would rob the whole system of all its potentialities. If the second method is chosen, a maximum could be worked out for each country, adjusted to its normal trade turnover, and it would be left to each surplus country to see to it that its own surplus did not become excessive owing to a deficiency of purchases.

The actual determination of these upper limits should not provide undue difficulties. The leading nations of the world would have to agree on some definite formula (e.g. 30 per cent. of yearly exports as the maximum overdraft) and then invite all the other nations to join in on these terms. For a start, pre-war trade would have to be taken as a standard. If the same formula is applied to all countries, there will be no occasion for special bargaining and detailed negotiations. As the trade of any one country expands or contracts, so will its maximum deficit be allowed to increase or decrease.[1]

Assume, then, that appropriate limits have been fixed for the deficit any one country will be allowed to incur, and the system is put into operation. It will work smoothly as long as every nation manages to avoid exceeding the deficit limits. In this respect it would be analogous to a Gold Standard system, which works satisfactorily as long as deficit countries manage to balance their foreign exchange income and expenditure in such a way that their gold reserve remains intact. What forces, if any, are operative within the system to facilitate this task?

The main force is the fact that the holding of surpluses becomes unprofitable and risky. The surplus, instead of being convertible into gold or interest-earning investments, is tied up in the Pool: it is a share in the Pool. And the Pool's assets are always the weakest currencies of the world: the currencies of the countries that have been unable to earn as much as they have spent.

In contradistinction to a bilateral set-up, however, Pool Clearing gives to each country the fullest opportunities to avoid becoming a surplus country: it can allow its importers to buy freely anywhere in the world, without regard to bilateral trade balances. If Argentina has made particularly large deliveries to Great Britain she can avoid becoming a surplus country by increasing her purchases (say) from the United States. This, of course, does not solve the problem of the British deficit, if there is one. But it solves Argentina's problem. The point is that each country can with the greatest ease and freedom avoid the accumulation of a surplus and reduce it once it has come into existence. If Argentina were tied down to spending all the proceeds of her exports to Britain on British goods, she might experience genuine difficulties in finding a sufficient amount of suitable and competitive goods on the British market. But if she is entirely free to spend her money wherever she likes, then any failure to spend on imports as much as has been earned on exports can only be a failure of effective demand within Argentina.

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[1] It is not suggested that there should be a *new* overdraft every year. The "maximum overdraft" might be considered as a *revolving* credit, the size of which fluctuates with the yearly volume of exports.

Such failures, of course, can occur, and are even likely to occur. Under the Gold Standard (or any other international monetary system based on free convertibility) they led to the general pursuance of a beggar-my-neighbour policy. Most countries during the 'thirties strove desperately to make up for the deficiencies of home investment by export surpluses ; those who succeeded reaped a double advantage : they increased home employment and accumulated either gold reserves at home or capital investments abroad. Under Pool Clearing a similar trend would undoubtedly assert itself, if the participating states failed to achieve a constant high level of employment at home. But it would take different forms. The surpluses arising out of current trade could not be converted into gold or interest-earning assets. They would be held as a precarious and barren investment. It might be, of course, that this deterrent against surpluses, or this incentive to import, would prove insufficient in a severe home unemployment crisis. But at least it is a force in the right direction which is absent from most other systems.

It might be said that any discouragement of surpluses will lead nations to ration their exports rather than expand their purchases. But this is unlikely. The temptation to aim at export surpluses is greatest when there is home unemployment. To restrict exports voluntarily just then would not recommend itself to any nation, and, if export surpluses are made unattractive by Pool Clearing, the chances are that a way out will be sought rather through speeding up imports by an expansionist internal policy (possibly with government help or on government account) than through slowing down exports. If this is so, the system would produce a strong inner tendency towards the expansion of world trade and would thereby stand in contrast to the system which prevailed before the war, when every international trade disequilibrium immediately set in motion restrictive forces.

The aim must be to achieve balance in the trade of every nation.[1] Such balance can be most easily achieved if potential surplus countries are discouraged from achieving a surplus. An attempt to throw the burden of adjustment primarily on the shoulders of the deficit countries (as in the past) is bound to fail, or at least to lead to competitive trade restriction. A surplus in one country leads to deficits in other countries as light produces shadow. Since it is on the whole easier to spend additional amounts than to earn additional amounts, any new system of international trade and exchange should be so designed that all its inherent forces induce the surplus countries to dissipate their surplus, rather than inducing the deficit countries to make a (probably fruitless) attempt to balance their accounts either by forcing their goods upon an unwilling world or by restricting their purchases.

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[1] Long-term lending will be dealt with below.

It may therefore not be rash to assume that Pool Clearing would exert a considerable influence upon the surplus countries to try and avoid all the risks involved in holding a share in the Pool by adopting any such measures as will increase the activities of their importers. But there are limits even here. It may well be that one or several countries are simply unable to offer, at the right price and at suitable delivery dates, a sufficient quantity of goods to pay for all their purchases. The evidence of this would be that their deficits showed a strong and apparently irresistible tendency to rise to their limits. It is in the interest of the rest of the world as well as of the countries concerned that the maximum limit should not be reached, because otherwise exports to the "overdrawn" countries might have to be rationed by direct intervention.[1]

In a situation of this kind there are, roughly speaking, three ways of escape that might recommend themselves :

> (1) The deficit country might be given loan. Failing that, it might be found (2) that the best way to rectify the situation would be a devaluation of the deficit country's currency. If devaluation is inadvisable, (3) some fundamental rearrangements of the internal structure of production of some of the countries concerned might be called for.

(1) The granting of loans, of course, is always the easiest, though not always the most satisfactory way out. In the case of "backward areas" that stand in need of industrialisation, in all cases of genuine economic development, loans should be resorted to. But an attempt should be made to distinguish such cases clearly from others where the urge to borrow and the urge to lend arise merely out of the desire of creditor and debtor country alike to postpone the carrying out of necessary adjustments in their respective structures of production. Balance should be the watchword. International loans, which make possible a (justifiable) long-term lack of balance, should be treated as the exception. This might be considered too much to ask for. But however that may be, without adherence to some strict principles of this sort, no system, whatever its technical design, can do lasting service. No system can ever enable some countries to follow a permanent policy of buying from the rest of the world more than they

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[1] In this case, the system, as far as the trade of the "overdrawn" country is concerned, would probably degenerate into some form of Bilateralism.

sell. Nor can it enable other countries to follow a permanent policy of selling more than they buy, and at the same time guarantee the creditor his money back, when he (or his country) is unwilling to accept it in the form of goods and services.

How, then, could the "exception", i.e. capital movements for the purpose of industrialising backward areas, be worked into the system of Pool Clearing ? It would be incompatible with the fundamental principles of the system, if Clearing Fund balances that have arisen out of past business were to be consolidated into long-term interest-bearing indebtedness. Credits should, on principle, be given only in connection with *new* deliveries to the deficit country. The difference between a conversion of existing clearing balances and making new deliveries on credit may appear slight. If a deficit country gets dangerously near its maximum deficit limit, does it not come to the same whether it transforms a part of the Clearing Fund balance into a bonded debt or obtains current imports on credit ? Assume that Poland has incurred a large deficit ; the Polish Government (or possibly even some private firm or institution) takes up a loan in U.S.A. ; the American creditor pays dollars into the U.S. National Clearing Fund, and the Polish National Clearing Fund disburses a corresponding amount of zlotys to the Polish borrower. This would be a purely financial transaction, with the result that the United States could sell more without increasing the internal indebtedness of her National Clearing Fund (which represents her export surplus and her share in the Pool), and Poland could buy more without increasing the cash balance of her National Clearing Fund (which represents her import surplus and her indebtedness to the Pool). If this is admissible, then the principal feature of Pool Clearing is marred : the pressure which the system exerts upon potential surplus countries to dissipate their surpluses by increased purchases is relaxed.

There appear to be two ways in which this problem could be handled. (*a*) Let us assume that the rule is adopted that all "financial" lending operations under Pool Clearing are prohibited. If a country (say Poland) is in need of a larger amount of foreign goods and services than she can immediately pay for by means of her exports, she might obtain a foreign credit "in kind", that is to say, she would ask some country or countries abroad to let her have certain specified deliveries on a deferred payment basis. Poland would then obtain goods without being obliged to make payment into its National Clearing Fund *at once*. Instead, she would issue a bond specifying the interest and amortisation payments that will be made later on. These latter payments, when they fall due, would of course take the form of money and would be made into the Polish National Clearing Fund, like any other international payment admitted under the scheme.

An arrangement of this sort would have the advantage of making the processes of international capital movements far more conscious and more easily recognisable than they have been hitherto. New borrowing would be for specified purposes only and take the form of the importation of goods (which might be producers' or consumers' goods) without immediate payment. Interest and amortisation charges would appear as a mortgage on the future overdraft facilities of the debtor country. The objection that might be raised is that in this way the lending operation itself (although not the return flow of interest and amortisation) would become bilateral in the sense that Poland, e.g., could not borrow in the United States and then spend the proceeds on additional imports from Britain. It is difficult to estimate the weight of this objection.

(*b*) Alternatively, the lending might take the form of an agreement between the U.S. Clearing Fund and the Polish Clearing Fund, whereunder additional imports made by Poland up to a specified amount are not to be paid for by the Polish importer through the Polish Fund, but by the American creditor through the American Fund. This arrangement would give Poland freedom to expend the loan money anywhere in the world. The objection that might be raised against it is that it would lend itself too easily to misuse. The weight of this objection, again, is difficult to estimate.

Whichever of these two alternatives is chosen, the effect will be that in a formal sense the debtor country will always be able to discharge the legal obligations which arise out of its indebtedness. Such payments will increase the cash balance in its National Clearing Fund and might raise it to the upper limit specified under the general scheme. They would act in the same way as payments made for additional current imports and create the same problems of adjustment. Similarly the National Clearing Fund of the creditor country would make corresponding payments to the creditor, thereby possibly increasing its own indebtedness to the internal money market—increasing, in other words, its share in the Pool. They would act in the same way as payments disbursed for additional current exports and create the same problems of adjustment. It would then be up to the creditor country (assuming that it is also a surplus country) to avoid becoming too large a holder in the Pool, by increasing its foreign purchases anywhere in the world.

This arrangement would show quite clearly the essential nature of international capital movements; they always are, and must be, an exchange (from the debtor country's point of view) of import surpluses to-day against export surpluses in the future. They are economically sound wherever there is reason to expect that the debtor country can increase its *future* capacity to export by increasing its *current* imports. If import surpluses are incurred without developing internal productivity and capacity to export, then existing maladjustments are not resolved and their solution is merely postponed. Escapist policies of this sort cannot be made impossible by any technical system; but it might be found that the arrangements suggested here will serve to clarify the position sufficiently to make their adoption less likely.

(2) We now come to the second way in which an adjustment might

be sought when a country appears to be unable to sell enough to pay for its purchases: devaluation. There is normally *some* level for each currency at which exports and imports will balance. But here again, " balance " is not everything, and equilibrium rates of exchange in many cases may mean disequilibrium somewhere else. They might, for instance, bring such a deterioration in the country's " terms of trade " as to be unbearable. But these are probably special cases. On the whole, a correct adjustment of exchange rates must be considered of the utmost importance. The principal criterion for correctness, and the object in view when changes are made, must be the establishment of " balance ", i.e. the avoidance of undue cash holdings or debits in the various National Clearing Funds.

In this connection it should be realised that, under Pool Clearing, the determination of the rates of exchange arises out of the co-operation between the different National Clearing Funds, and no one country is able to settle the rates of exchange of its own currency in opposition to the rest of the world, as has been possible hitherto. Clearing Fund *A* receives a certain amount of its own currency from an importer who has bought goods from an exporter in country *B*. Clearing Fund *A* thereupon advises Clearing Fund *B* of the payment received, and it is up to the latter to pay the exporter *at the ruling rates of exchange*. Thus, in order to alter the value of its currency, Clearing Fund *A* must first seek agreement with all other Clearing Funds, so that they will actually make their disbursements to their respective home exporters at the new rates.

Is it likely that such international agreement on exchange rates will be obtained? This question sounds more formidable than it really is: under *any* system—even in the absence of an organised system, there must always be agreement between those who exchange currencies. Nobody proposes to leave these dealings entirely unorganised, and governments the world over are already accustomed to taking a hand in them. To get some working agreement to start with, therefore, will not be too difficult. Once a start has been made, it must be left to the actual development of trade (and lending) and to a process of trial and error to find those rates of exchange which most nearly establish an equilibrium position between the different countries. That this cannot be found, and that the necessary revisions which changing circumstances demand cannot be carried through, without some organised international effort, should by now be abundantly clear. There will be Exchange Equalisation Funds, no matter what system is finally adopted. And these institutions will get into touch with one another to settle policy on a wider or smaller scale. Pool Clearing does not *add* to the difficulties inherent, at the present stage of world political development, in any effort of international co-ordination. In fact, the Pool might be looked upon merely as an Exchange Equalisation Fund on a world scale. The inner mechanism of Pool Clearing, moreover, itself provides time and inducement for co-operation. It becomes unattractive, as we have seen, to accumulate surpluses ; but it becomes also unattractive to all participants to allow the deficit of any country to rise dangerously near its maximum limit. It is in the interest of both potential surplus countries and potential deficit countries to find an equilibrium level of exchange. Hitherto this was not the case. No matter how hard deficit countries might have been striving to reach equilibrium rates, the surplus countries found it useful to hang on to their surpluses and to answer the devaluation of other currencies by a devaluation of their own. Competitive devaluations, under Pool Clearing, would be completely pointless.

Assuming, then, that each country accepts the duty of keeping its own Clearing Fund in balance, and grants the right to every other country to choose any such means as will serve this purpose in their own case, there should be some international agency which will provide the machinery for reaching common decisions. The International Clearing Office, proposed above, might fulfil this function. But this would imply, of course, that its power and status would have to be raised far beyond that necessary for carrying out the purely formal " trustee " functions of " pooling ", as they have been described.

Our choice in this matter is not very wide : we can leave the conduct of economic affairs to a multitude of sectional interests (be they private or " national "), in which case we shall continue to be at the mercy of forces which have in the past landed us in a series of economic and political disasters ; or we can make an attempt at some form of world planning. Such an attempt may be successful, if the strongest nations take a determined lead and accept responsibility not only for the well-being of themselves but for that of the world as a whole.

All economic phenomena, in an era of quick transportation and intensive economic exchange, are interconnected. It would be foolish to propose that everything should be controlled centrally. But international trade is of fundamental importance as a strategic point of control. It might, therefore, be worth considering whether the time is not ripe for some international control in this field. The object of control, it is suggested, should be merely to achieve balance in the Clearing Funds of the various nations ; to avoid long-term cumulative surpluses and long-term cumulative deficits.

This, of course, would have far-reaching effects also upon purely internal policies in the different countries. Internal monetary policies, for instance, exert a strong effect upon external trade. A proposal, however, to subject them to international control would at the present stage appear wholly utopian. Thus each nation should be free to do internally whatever it pleases ; to aim at full employment with all its might ; to distribute its national income according to its own principles or lack of principles ; to control foreign exchange transactions or to leave them uncontrolled ; etc. Its responsibility to the rest of the world should end when it has balanced its Clearing Fund accounts. The primary responsibility should rest on the shoulders of surplus countries to spend as much as they have earned; and a secondary responsibility should rest on the shoulders of the deficit countries to make available, at the right prices, etc., a sufficient volume of goods so that all cash obligations can be discharged in kind. If an adjustment of the rates of exchange appears conducive to the discharge of these responsibilities, the International Clearing Office, as the responsible international agency, should seek to arrange it.

(3) In some cases the degree of devaluation necessary for " balance " may be such that vital interests of the country in question will be endangered. It is then that international loans might be considered, but they have been dealt with above. With the help of loans, or without it, the country would have to reorganise its industry, which would often mean a change over from home production to exports, or from one kind of export goods to another, or else a curtailment of its purchases. But a reorganisation within the deficit country alone might not solve the problem. An equal duty falls upon the surplus countries to reorganise in such a way that they can take more goods if more are offered. In such cases it may sometimes be unavoidable to adopt a certain degree of Bilateralism. A country that changes its structure of production so as to be able to export more must know the market whose requirements it is hoping to meet. And a country that makes similar internal changes so as to create additional import requirements must know its sources of supply.

Adjustments of this kind are admittedly difficult to effect, and even more difficult to enforce. But these difficulties again are not peculiar to Pool Clearing. They are inherent in any attempt at international economic co-operation, whether for the control of raw materials, cartels, transport, or anything else. It is often a matter merely of " Economic Intelligence ", of the spread of reliable knowledge, and the compilation of reliable statistics of productive capacities and human needs. A clear recognition of the necessity of balance, together with a system of exchange which makes surpluses unattractive, might exert pressure in the right direction. But it must be emphasised that no system, whatever its technical form, is likely to endure unless the great cyclical waves of unemployment, which have been the greatest single factor of disruption in the past, can be avoided in the future.

In the beginning of this essay we have accepted, by way of assumption, that certain advantages could be gained by employing the clearing principle for international trade. The course of our discussion should have made it plain that this assumption is not entirely unjustified.[1] In comparison with free international convertibility of currencies, Pool Clearing offers at least one outstanding advantage: that its inner mechanism tends to overcome temporary disequilibrium situations in international exchange by expansion instead of restriction. The buyer is given the first move in the game, on the self-evident theory that one country's imports are another country's exports and that, if only demand can be liberated, supply will largely take care of itself. Thus the main burden of adjustment is placed on the surplus countries, and forces are brought into play to make them spend their surpluses, thereby enabling the deficit countries to get rid of their deficits without restricting their purchases. The overdraft facilities to be granted to each country are an expression of the determination to give the buyer the benefit of the doubt, and a means of getting international trade started again after the war, in spite of the widespread shortage of international cash.

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[1] For a further discussion of the clearing principle see: E. F. Schumacher, " United States Foreign Trade Policy," *Agenda*, Vol. 1, No. 3, July, 1942.

This inner expansionist force of the system is its most important feature. The isolation of trade movements from speculative capital movements, which this system brings about, can be attained also by other means. So can the medium-term stability of exchange rates, or the minimum degree of international co-operation which is necessary in this field whatever its technical organisation. The same applies to the fact that Pool Clearing would make the international payments position of each country " transparent " by summing up its vital aspect in *one* figure, the debit or credit balance of the National Clearing Fund. All this can be attained by a variety of means, not necessarily those of Pool Clearing. But can we get this expansionist element into the international system by other means ? And is this element something worth striving for ? These are the questions which grow out of this essay and towards the solution of which the above is hoped to be a contribution.

Whether the efforts required for the establishment of such a system are worth while, and whether the risks inherent in any new departures can prudently be taken—that can be decided only by considering the available alternatives. At least two alternatives are at present clearly visible : a continuation of the pre-war trend which worked towards a splitting up of the world trading system into innumerable uncoordinated bilateral systems. This prospect needs only to be stated to reduce itself to absurdity. Or : a continuation of the war-time trend towards the formation of regional economic blocs, within which, in all probability, some form of multilateral clearing would ultimately establish itself.

Economic regionalism might be considered superior to the anarchy of rigid Bilateralism. But it surely cannot be the last word in post-war economic organisation. The above attempts to show just one way of driving a wedge into the solid block of resistance against economic planning on a world-scale. Other ways, no doubt, exist. They should be fully described and fully discussed. Some world-embracing scheme, conceived with realism and daring, must be developed to meet the challenge of Bilateralism as well as that of Economic Regionalism. And such a scheme, it is felt, should above all possess an inherent tendency towards expansion.

# Public Expenditure in the National Income

By Ernest H. Stern

The calculations of the National Income provided in recent years by British and American authorities and scientific institutions are such valuable achievements that I do not feel happy in exposing certain serious mistakes committed in both sets of calculations. In the National Income calculations both of the U.K. and U.S.A. national Income is conceived as the aggregate money value—at current prices—of all goods produced and services rendered less the goods and services required for the maintenance of the earning power of the nation, the latter for simplicity's sake being identified mainly with the maintenance of the durable tangible assets, whether these are for productive or consumptive purposes. The National Income of this concept is equivalent and, provided the statistics are accurate, equal to the aggregate of individual incomes derived from any form of economic activity, private or public, plus undistributed business profits (or minus losses), i.e. to the total of personal incomes, other than mere transfer incomes, and of the surplus of enterprises, private or public, that are available for comfort and saving, comfort being the consumption of food, shelter etc., clothing, services of health, education and recreation. Because the statistics of personal and business incomes received are more readily available than those of the net aggregate money value of goods and services produced and sold, the method of adding up the incomes and making allowances for the maintenance requirements have hitherto been applied all over the world for the calculation of the National Income. However, the application of this method has been subject to a significant error in both the calculations of the U.K. and U.S.A. For brevity's sake, this article deals only with the British calculations in the White Paper Cmd. 6347/1942 on "An Analysis of the Sources of War Finance and an Estimate of the National Income and Expenditure, etc."[1]

The broad principles of method followed in the White Paper have been described by J. E. Meade and Richard Stone in their article in the *Economic Journal* on "The Construction of Tables of National Income, Expenditure, Savings and Investments".[2] The authors of the White Paper calculate the net National Income as the aggregate of all individual and institutional incomes, other than those derived from the interest on the National Debt, war pensions, social pensions and charitable assistance (regarded as transfer payments), plus Government trading profits and related income minus income due to non-residents. By excluding the income classified as transfer payments they realise that the benefit of one and the same economic achievement is shared in several stages by several persons and that if the income received at these several stages were all allowed to enter the account one economic achievement would be counted several times. The authors of the White Paper also exclude private and business expenditure required for keeping intact the earning power of the sources of income. On the other hand, they include the income derived from all public expenditure such as received by servants of, or suppliers to, public authorities. They do not contemplate the possibility that part of the public expenditure may be made for the purpose of maintaining the earning power of the national community. They tacitly assume that public expenditure, as distinct from private expenditure, represents only services and supplies such as increase the comfort or the savings of the Nation and that none of them is consumed in safeguarding its earning power. This assumption is in no way borne out by the facts either in peace or, evidently, in war. In this respect, therefore, the National Income calculations of the White Paper want correction.

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[1] Hereafter, for short, called the White Paper. (Since the going into print of this Article, a new White Paper on this subject has been published (Cmd. 6438/1943) which repeats the methods of the White Paper 1942. All comment made in the present Article, therefore, applies equally to the new White Paper.)

[2] June/Sept., 1941. p. 216 *seq.*

However, the authors of the White Paper must have felt that income derived from public expenditure, which itself is derived from taxes, may be counted twice, if taxes were not deducted from the income. At any rate, they did deduct taxes; however, not all of them, but only those of an indirect nature.[1] Meade-Stone (*op. cit.*) call the result the "Net National Income, at factor cost". They justify both method and choice of term by explaining that the income thus recorded represents the income received by the factors of production from the sale of their products or services, because out of the actual receipts from their sale in the market an amount equal to indirect taxation is handed over by them to the tax collector. In their explanation the National Income not thus adjusted for indirect taxation would represent the "Net National Income, at market prices". They apparently, though tacitly, assume that the money collected by indirect taxation is taken out of the circulation. Nothing of this kind happens. The facts are :—

1. The money raised by indirect taxation reverts to the economic community in exactly the same manner as money raised by direct taxation and is spent on services rendered and, therefore, included in the income of the factors of production in exactly the same way as the money raised by direct taxation.

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[1] "Indirect Taxes" in this article means—for brevity's sake—such taxes as are included in items 11 to 13 in Table I of the White Paper.

2. The income received by the factors of production is always recorded at " market prices " and their sum total can be no other than total income at market prices. The deduction of indirect taxes would make it into a total " at factor cost " only if the indirect taxation were equal to the costs of distribution, which it obviously is not, and is not claimed to be by Meade-Stone.

A National Income including the services of the distributive trade, as is that of the White Paper, is invariably " at market prices " ; it can never be at factor cost. The authors of the White Paper and Meade-Stone could not have failed to see this if they had conceived the public authorities as what they actually are, i.e. as employers, buying services and commodities in the market and paying for them at market prices. They would then have perceived that the recipients of that income at market prices are at the same time, though in varying extent, the providers of it and that, therefore, the same income may be counted twice. In fact, some of it is being counted twice in any total that is " before taxes " and, therefore, in the White Paper Net National Income, which is " before direct taxes ". The " Net " National Income of the White Paper, by Meade-Stone termed " at factor cost ", is neither at factor cost nor net, nor gross ; it is at market prices somewhere between net and gross.

However, not all public expenditure represents double counting. Some services performed by the public servants add to the mass of goods available to the public for consumption or saving. Which do, and which do not, will be explained in more detail in Section II. Only the latter represent double-counting. There is no distinction in this respect between the income equivalent of public and of private effort.

*The mistake of the authors of the White Paper and Meade-Stone lies in their choosing as criterion—for the elimination of double-counting—the mode of taxation instead of the function of the services performed by the recipients of the taxes.* There is in this country in our days no connection between the manner of the collection of taxes and the economic quality of the services performed by public authorities.[1] I propose to correct this mistake and to calculate the National Income by adding up all incomes not of a transfer nature and by deducting all income derived from services which are of a cost of production nature irrespective of whether these services are rendered in the private or the public sector.[1] There exists no other Net National Income.

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[1] In the calculations of American National Income by Simon Kuznets—and apparently also in those by the U.S. Department of Commerce—the adjustment is, as in the White Paper calculation, made by deducting certain taxes from the total. But in the American case the taxes, though styled " Indirect ", exclude some which in the White Paper rank as Indirect taxes (as e.g. employers' social security contributions) and include others which in the White Paper rank as Direct taxes (as e.g. Business Income Tax and E.P.T.). Kuznets assumes the taxes borne by business firms, whether direct or indirect taxes, to be more or less the equivalent of the services performed outside the sphere of consumption. Kuznets is aware that this assumption may well be wrong and his calculations equally wrong. For him his choice is not a correct method but a lesser evil in the face of difficulties of compilation. (Simon Kuznets : *National Income and its Composition, 1919–1938*, Vol. II, p. 428 *seq.*).

## II

For the purpose of making the function of the services rendered by public authorities the criterion of adjustment, the public expenditure may be divided into four main sections, viz. :—

(*a*) Expenditure for the attainment of security, internal and external, and the smooth flow of the life of the community (for short called "security expenditure"). It comprises the expenditure of the Consolidated Fund, other than interest, of the Home Office, the Armed Forces, Foreign Office, the corresponding departments of Local Authorities and any other expenditure for the maintenance of security, law and order.

(*b*) Expenditure for services which add to the comfort and personal improvement of the citizen (for short called "comfort expenditure"). It comprises public expenditure for education, health, improved housing, recreation and the amenities of life.

(*c*) Expenditure for services which add to the earning power of the sources of income (for short called "savings"). It comprises the net increase of tangible assets, such as the durable equipment of the nation and its foreign investments, but may also include intangible assets, of which more in Section III.

(*d*) Expenditure of a transfer nature, such as interest on public debts, etc.

As soon as the attempt is made to arrange the public expenditure under these four headings, a number of border cases will come to light, as they are bound to arise whenever social phenomena are classified. I will deal presently with a few border cases, for the sake of illustrating the functional differences, but, firstly, I wish to explain the bearing on the National Income of these functional differences :—

(1) *Security expenditures are in the nature of "costs of production" and, therefore, form no part of the net National Income.*

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[1] The same suggestion has been made as regards the American calculations by Dr. G. Colm in his paper on "Public Revenue and Public Expenditure in National Income" in the symposium on *Studies in Income and Wealth*, Vol. I (Nat. Bureau of Economic Research, New York, 1937). In the debate which followed, his suggestion though shared by H. Newcomer was rejected by S. Kuznets and others, and in fact has not been adopted either by the National Bureau of Economic Research (Kuznets) or the U.S. Department of Commerce. A similar line had previously been taken by the Central Statistical Office of Germany (*Das Deutsche Volkseinkommen vor und nach dem Kriege*, Berlin, 1932). As has been explained in Footnote (1) on page 168, Kuznets though not changing his methods, for reasons stated, has now acknowledged the correctness of the principle, at least that adjustment should be made in accordance with the function of services rendered.

The authors of the U.S. Department of Commerce calculations have not, to my knowledge, made any such acknowledgment, but when using the unadjusted total were at pains to leave it unadjusted for both private and public expenditure of a depreciation nature and to call it "gross national product at market prices", as distinct from the White Paper term "Net National Income at market prices". (Milton Gilbert, "War Expenditure and National Production," U.S. Department of Commerce, *Survey of Current Business*, March, 1942, page 10).

They are pre-requisites of, and instrumental to, attaining a certain level of production or safeguarding the earning power of each productive unit of the nation. The position is made clear by reflecting on a situation in which the maintenance of security, law and order is undertaken by business companies or is left to the independent efforts of each individual. The costs of the guards required for preventing burglaries, for quelling riots, for the arbitration of disputes, or for escorts accompanying the transports on the highways and on the seas, the costs of the private fire brigades, the maintenance gangs on roads, all this would rank in their balance sheets or tax returns or census returns as costs of production and be excluded from the aggregate income. It is not so long ago that a great deal of such services were, in fact, performed in the private sector and a certain amount of services of a like nature have been provided in the private sector even in the days of the present generation, especially in the U.S. and on the Continent of Europe. It makes no difference to the National Income whether those costs represent payments to members of factory guards or farm patrols or subscription fees to joint companies patrolling roads, property or the seas or costs of escorts through the deserts or even fees to racketeers or taxes to the paramount power, which in this country is the Government, but in Fascist countries is shared between Government and Party. Military expenditure is an outstanding example of such security expenditure—and the tanks lost in battle, the ships lost on the seas, the shells fired into the air and a great deal of other expenditure certainly form no part of the Net National Income, i.e., no part of the annual economic effort available for comfort or saving. The very costs of tax collection are another typical example of the "costs of production" nature of certain public expenditures.

(2) *Comfort expenditure and savings, on the other hand, are part of the Net National Income.* It makes no difference to the total of the National Income whether education is provided by private schools and the fees for the term paid out of the income of the individual or by state schools and the costs collected in the form of taxes; whether theatres are self-supporting or dependent on donations or on state subsidies, nor does it make any difference whether the contribution to health insurance is voluntary or compulsory, or whether the costs of medical treatment are paid outside any health insurance or the hospitals living on charity or on taxation. All these expenditures are of the same nature, i.e., for services which add to our comfort or earning power and are to be paid out of the income, as it is understood in the generally accepted concept of the National Income.

However, there are border cases. In fact, any calculation of the National Income bristles with border cases owing to the very concept of National Income. Opinions may differ as to whether the costs for elementary education or for the training of skilled workmen belong to the " costs of production " category or to the " comfort " category. Even if elementary education were to be regarded as of a " costs of production " nature, free milk supply or medical supervision in schools would still be comfort expenditure and part of the National Income. Whereas the expenditure on uniforms, camps, munition dumps, raw material stocks, aircraft, lorries, etc., are of a " cost of production " nature, the value of the equipment left over at the end of the war and serviceable in peace time forms part of the National Savings and, therefore, of the National Income. Although in this country the Government are mainly engaged in providing security, some Ministers are, however, largely, and the Government as a whole partly, concerned with the provision of comfort services. And so on and so forth.[1] Those who have access to the details of public expenditure will be faced with no insurmountable difficulties in deciding the nature of any expenditure. If the National Income has been arrived at " before taxes " (direct or indirect) the adjustment is to be made by deducting the security expenditure; if " after taxes " (direct or indirect) by adding the Comfort and Savings expenditure.

## III

So far it has been tacitly assumed that the public expenditure has served only the one year during which it was expended and that similar expenditure was recurring annually. Actually a number of payments are not annually recurring, but provide services, the benefits of which are derived over a number of successive years. Non-recurring services, the benefits of which extend over several years, are to be allocated in parts to the years they serve and consequently only a fraction of the original payment to the year in which it was collected or spent. This way of accounting has long been adopted in business and in many public accounts; it acts on the accounts as if the total non-recurring expenditure were capital, intangible assets as it were, and one part amortised every year. Inasmuch as " Comfort " expenditure is concerned this makes no difference to the total National Income; it only turns a part of the " Comfort " items into " Savings ". It makes a great difference, however, to the total National Income where security expenditure, or any other expenditure to be written off, is concerned.

War expenditure is one, and at the same time, the most outstanding example of such non-recurring expenditure. Hence it follows that in the National Income account the current war expenditure (security category), instead of being written off in the year it was spent, is to be credited to the income account and at the same time an appropriate instalment of the total war expenditure (security category), i.e., of the expenditure of several years if the war extends over several years, is to be debited to the income accounts of each year from the beginning of one war to the beginning of another war.

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[1] When dealing with the " burden " of taxation G. L. Schwartz has drawn attention to many more border cases, especially those occurring in the group of " War Expenditure " (*vide* his paper on " The Significance of Fiscal Statistics ", *Journal R.S.S.*, 1942, II).

The principle is clear, but the translation into practice is subject to uncertainties inasmuch as the total expenditure to be spread and the number of years over which it has to be spread have to be anticipated. However, similar uncertainties are met in commercial accounting as well, rates of amortisation of equipment always being subject to estimates of future events. They have not been found insoluble for practical purposes, and the estimates incorporating the solutions are the basis of the National Income calculations. Although some of the estimates in business can be based on past experiences, many of them cannot, and in consequence may differ by as much as 100 per cent. In proper commercial accounting the rates of amortisation are continuously revised post factum. Difficulties like these are inherent in all income accounts. If they destroy the appearance of statistical exactitude of National Income calculations they serve moreover a good purpose by making it evident to a wider public that the latter are not more than approximations.

The introduction of the gradual amortisation of non-recurring expenditure being a new departure in National Income calculations, it is appropriate to explain the main principles that are to be followed in determining the rates of amortisation. Three questions are to be answered :—

(*a*) The total amount of non-recurring expenditure to be allocated ?
(*b*) The number of years over which the amortisation is to be spread ?
(*c*) Is the allocation to be made in equal parts or in differing parts, adequate to different benefits derived from the non-recurring expenditure in various years ?

The reply to (*a*) and (*b*) is that in the case of war expenditure the total expenditure (security category) of the whole war, including that part of the reconstruction expenditure that serves the making good of war damages, has to be allocated over the combined period of the war and the peace that follows. If the National Income is to be calculated before the war has ended, an estimate has to be made of the duration of the war and the expenditure that is going to arise in the following, say, second, third and fourth years. Assume the expenditure in the first year to be 1,000, the second 2,000, the third 3,000 and the fourth year 4,000, then the total expenditure to be allocated over the period that begins with the first year is not 1,000 but 10,000, and in the second year not 3,000 but once more 10,000 and so on, and the number of years over which the 10,000 are to be spread include the years of war and the estimated period of peace to follow. Whether the allocation should be in equal instalments or in changing instalments depends on the merits of each individual case. For the sake of illustration, one instance may be given. Assume the total expenditure to be £10,000 Mill., the period during which it is spent and benefits are derived to be 28 years (four years of war and 24 years of peace), the National Income before writing off war expenditure to be £5,000 Mill. per annum in the first of the allocation period and to rise at a steady cumulative rate of 2 per cent. per annum. In this case the benefits derived from the security obtained by the war expenditure would rise annually, and consequently the annual instalments should not be equal but rising at a cumulative rate of 2 per cent., causing the amortisation to remain an equal proportion of the unadjusted income. The instalment to be written off the unadjusted income of the first year of the period would be approximately £264 Mill., and the last instalment to be written off the unadjusted income of the 28th year would be approximately £460 Mill., the quota of amortisation being 5.3 per cent. of the income before amortisation every year over 28 years. In actual circumstances more refinements may be required, but the above example may suffice to illustrate the principles and their application.

It may be argued that because of the unpredictability of the recurrence of war expenditure the commodities and services which are consumed in the process of warfare, i.e., in an activity of a non-productive kind, should be written off in the year of consumption. Although I do not believe this argument to be justified, holding the opinion that external security is a valuable economic asset, albeit of an intangible nature, increasing or safeguarding the net earning power of the nation, there is nothing to be said against testing the effect of the contingency of war expenditure on a National Income.

## IV

The correction suggested may be summarised in the following equations :—Let $A$ stand for net income derived from private expenditure, before direct taxes ; $B$ for net income derived from public expenditure before direct taxes ; $Td$ for direct taxes ; $Ti$ for indirect taxes ; $Sr$ for annually recurring public expenditure of the " Security " category ; $Sn$ for non-recurring public expenditure of the " Security " category ; $\frac{1}{Q}$ for the quota of amortisation of $Sn$ ; $Z$ for transfer payments which include $F$, standing for subsidies, in addition to those included in the transfer items in the White Paper, then—

(1) Net National Income at market price

$$= A + B - Z + Ti - \left(Sr + \frac{\Sigma Sn}{Q}\right)$$

(as suggested in this article)

(1*a*) —ditto—if non-recurring expenditure allocated as it occurs $= A + B - Z + Ti - (Sr + Sn)$

(2) Net National Income in the White Paper
("at factor cost" in terms of
Meade-Stone) $= A + B - (Z - F)$

(3) Net National Income "at market price"
(in terms of Meade-Stone) $= A + B - Z + Ti$

Apart from $F$, the inclusion of which in the White Paper "Net National Income" appears to be an error of application, not of principle, the equations (1) and (2) differ inasmuch as $Ti$ and $\left(Sr + \frac{\Sigma Sn}{Q}\right)$ differ from each other. There is no reason why these two magnitudes should be equal, though by good luck they may happen to be close to each other. Income in (3) is in excess of actual Net Income by $\left(Sr + \frac{\Sigma Sn}{Q}\right)$, which is always substantial.

The principles of accounting suggested in regard to the National Income apply equally in accounting for the National Expenditure and National Savings. The White Paper "Net National Expenditure" (Item 14, table I, White Paper) at factor cost in terms of Meade-Stone is actually a gross expenditure at market prices adjusted for some but not all double-counting. National Expenditure is always "at market prices" inasmuch as individual expenditure is at market prices. The Net National Expenditure is one adjusted for any expenditure incurred in the process of production, i.e., any expenditure outside the sphere of consumption (comfort) and saving, whether in the private or public sector.

If $C$ stands for personal expenditure on consumption; $E$ for total public expenditure; $P$ for public expenditure on comfort; $I$ for total savings (investment at home and abroad) in the private and public sector; $Ia$ for savings in the private sector; $Ib$ for savings in the public sector; $D$ for public expenditure on War Damage, etc., claims (item 28, table IV, White Paper); other symbols as above (National Income equations); then the Net National Expenditure can be expressed in the following equations:—

(1) Net National Expenditure $= C + P + I$.

As $P = E - (Sr + Sn + Ib + Z)$ and $I = Ia + Ib$;
the equation can also be read as follows:—
Net National Expenditure

$$= C + E - Z + Ia - Sr - Sn$$

It is evident that neither $Td$ nor $Ti$ have a place in the equation and that therefore none of them can be an adjusting factor.

(2) Net National Expenditure of
the White Paper (item 15,
table I, "at factor cost"
in terms of Meade-Stone) $= C + (E - D) - (Z - F) + Ia - Ti$

The exclusion of $D$ from the Net National Expenditure and of $F$ from the transfer items and therefore its inclusion in the Net National Expenditure appears to be an error of application, not of principle. The substitution of $Ti$ for $Sr + Sn$ is, however, an error of principle. As there is no reason why $Ti$ should be equal to $Sr + Sn$, unless accidentally, the White Paper calculation (item 15, Table I) cannot correctly reflect the Net National Expenditure, even after errors of application have been corrected.

National Savings equal the balance of the Net National Income and Net National Expenditure on Consumption (comfort), the latter irrespective of its occurring in the private or public sector. They may include Tangible and Intangible Assets, and if non-recurring expenditures are allocated only partly to the years in which they are spent, they do include Intangible Assets. The National Savings, expressed as an equation, may be calculated as follows :—

$$(1)\ I = A + B - Z + Ti - \left(Sr + \frac{\Sigma Sn}{Q}\right) - (C + P)$$

$$= A + B + Ti + Ib - (C + E) + Sn - \frac{\Sigma Sn}{Q}$$

*Ib* is *nil*, if, as usual, it is included in *Sn*, but there is no reason why in future it should not be separated from *Sn* in the accounts of Public Authorities. *Sn* has been assumed to have been *nil* in the pre-war period, but it has been very substantial during the war. In the case of non-recurring expenditure being allocated as it occurs, instead of to a series of years, the equation will be as follows :—

$$(1a)\ I = A + B + Ti + Ib - (C + E)$$

The equation of the White Paper reads as follows :—

$$(2)\ \ I = A + B - (Z - F) - (C + E - D - Z + F - Ti)$$

$$= A + B + Ti - (C + E) + D$$

The omission of *Ib* and the inclusion of *D* in the White Paper calculation of the National Savings appear to be errors of application, but not of principle, whereas the omission of $Sn - \frac{\Sigma Sn}{Q}$ is a matter of principle. $Sn - \frac{\Sigma Sn}{Q}$ being an Intangible Asset, the calculation of the Tangible Savings is incorrect only by $(Ib + D)$. However, as *Ib* is assumed to be *nil* in all years, and *D* was *nil* before the war, the calculation of the Tangible National Savings is correct in 1938 and those of 1940/41 too high by *D*, or rather in these latter years the Tangible National Dis-savings are understated to the amount of *D*.

# A Liberal New Order

### By Allan G. B. Fisher

Professor Hutt's latest book[1] is an important essay in institutional invention, being in fact nothing less than " an attempt at a systematic revision of the system of property ". He thinks that in recent years economists have been too ready to rest content with mere criticism of the restrictionist devices commonly adopted for protecting established expectations and thus assuring security, the fixation of minimum prices or maximum outputs or the limitation of recruitment, and have too seldom carried their destructive analysis the necessary further stage, which would show what actually ought to be done about it, to replace these self-frustrating devices, which not only destroy productive power, but also destroy the social security which it is their avowed object to ensure, by something better. They are accordingly not without a considerable measure of responsibility for the unfortunate state of affairs which has arisen as a result of the widespread adoption of these self-frustrating policies. " They have been dispassionate critics rather than social architects. They have been students of institutions rather than inventors of institutions. Whilst their teachings have, by implication, condemned all the manifestations of scarcity creation by which the world was ravaged in times of peace, whilst they have revealed the origins of poverty, insecurity and unemployment, and whilst they have exposed the countless misguided reformist efforts which have aggravated these evils, they have not themselves been disposed to compete in the production and sale of Utopias." Seven years ago, in *Economists and the Public*, Professor Hutt promised his readers a positive plan for the reconstruction of the " economic institutions required for an equalitarian or competitive society ", which would make it possible to release " the full equalitarian force of capitalism ". *Plan for Reconstruction* is now offered in fulfilment of that promise.

Especially in recent years, when reforms and plans of all kinds are being pressed upon our attention from many quarters, the common complaint that the inventive faculties of mankind have been exercised too exclusively in the field of the natural sciences, and that the development of the new social institutions, as instruments of social control, has been correspondingly neglected, might at first sight appear highly paradoxical. And it is indeed obvious that the social field has not been entirely neglected by the inventor. An analysis of our own political institutions during the inter-war period reveals something in many fundamental respects quite different from the political institutions with which our grandfathers were familiar. Nor has the business man been content merely to inherit the forms of organisation which his forebears had been accustomed to administer, while in the much disputed field where State policy and business activity interact the economic history of our generation can easily be presented as nothing but a long series of experimentation and invention.

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[1] *Plan for Reconstruction.* Kegan Paul, Trench, Trubner and Co., 1943. 328 pp. 18s.

Much of this however has been a mere matter of trial and error, and when we examine the current efforts of social and economic inventors, the position is often still less satisfactory. Apart from the fact that much of the current work of social invention is merely a refurbishing of old and often discredited ideas, containing little that is new, there is one other fundamental fact which distinguishes the work of the inventor in the physical or the chemical laboratory from many of the schemes which are offered to us to-day by social planners. No inventor in the former field would ever think it sufficient merely to describe the mechanical form of his invention. He would also take for granted an obligation to show how the physical, the chemical, the biological forces, which he proposed to harness, were to find an appropriate new outlet through his new device, and produce the result which he claimed his invention would make possible. Unfortunately the sponsors of a large number of the institutional inventions which are now being offered to the public show little disposition to follow the example in this connection of the inventor who is concerned with the " natural sciences ". The new institutional machinery is described with zest and sometimes in great detail, but the economic forces which are to work the machinery are almost completely neglected, except that it is usually made clear that references to profit are taboo. They seldom tell us anything precise about the principles by reference to which the decisions of the new institutional authorities are to be guided, and indeed display a certain impatience if the question is even raised. Even the purposes which the new machinery is to serve are not very precisely described, the vague references to the common good or the public interest which are often made to serve as substitutes being little more convincing than would a corresponding claim on behalf of a new drug that it was intended to raise the general standard of public health. For these reasons it is not unfair to say that many of the self-styled planners are not planners at all, but merely visionaries.

If new institutional inventions are to serve a useful purpose, it is important first to have a clear idea of the fundamental character of the defects of the existing institutional machinery. Lack of interest in the economic principles or forces which are to work the new inventions is however itself often a reflection of lack of understanding of these defects. It is generally agreed that something is radically wrong somewhere. It is easy to dilate upon the devastating consequences of social and economic disease, but its diagnosis is less easy unless one is prepared for a more strenuous intellectual effort than many social inventors seem disposed to put forth. This has the unfortunate consequence that practical men, many of whom too long refused to admit that anything was seriously wrong, and indeed often themselves have engaged in activities which made the situation worse, and who in any event are not much disposed by temperament to think in terms of fundamental principles, have often in recent years, now that they have been shocked out of their lethargic complacence, been too ready to take quack remedies seriously. Even if they have felt in their bones that there must be something wrong in such proposals, they have been unable to explain convincingly to themselves the ground for this belief, because they have failed to identify the real enemy which has threatened both economic progress and economic security. In their bewilderment they often dissipate their energies by continuing to wage the old battles which their grandfathers began, without realising that much more dangerous and insidious attacks have now developed, and sometimes indeed inside their own camp.

Professor Hutt's book is a bold attempt to work out, for British economic policy in the post-war period, a comprehensive and self-consistent plan which shall be free from the fundamental defects which characterise so much of the current "planning" literature. He sets out clearly the objectives which his proposals are designed to attain, and his position in regard to the economic principles and forces which are to provide the driving power for the new machinery and to guide the activities of the new authorities which he proposes to create is clear and unequivocal. He even goes so far as to set out in the first 60 pages of the book drafts for three bills, in which his proposals are given a formal legislative embodiment. He writes with a sense of extreme urgency, and his programme is far-reaching and radical. It is, as he claims, "*really* a plan and not a vision". Unfortunately, many of those whose attention it is most important to direct to his ideas will not always find his argument easy to follow, and though to many readers of ECONOMICA and of Professor Hutt's earlier work, some of it will already be familiar, the most useful service which it appears a reviewer can perform is to present in bare outline the essential features of his plan.

What indeed is the fundamental economic issue which in the modern world most urgently demands our attention? In Professor Hutt's view the real enemy against whom our fire should be directed is "restrained productive capacity". Throughout history the most formidable barrier to economic progress has been the clash between unrestrained productive capacity and the deeply-rooted sentiment in favour of the maintenance of established income expectations, a clash which has meant more and more in our own time that security has been sought by way of restraint of production. But if the real enemy is " restrained productive capacity ", from whatever quarter the restraint may be imposed, much of the old controversy about private enterprise and state enterprise is pointless and irrelevant. There is nothing in their nature which would prevent some of the current proposals for state activity in the economic field from being just as restrictive of production as the old private arrangements which they pretend to supplant, and indeed they are sometimes quite candidly defended for that very reason. Equally the controversies about monetary policy, which have been so fashionable in recent years, are of quite subordinate importance, unless the problems with which they deal are put in their proper place within the wider framework of what should be regarded as the overriding purpose of economic policy, " the ultimate removal of hindrances to productivity ", an end with whose realisation the defence of the returns of particular enterprises is often quite inconsistent.

In Professor Hutt's view, the price and profit mechanism is the only possible instrument for indicating the direction and magnitude of appropriate changes in the allocation of productive resources. In no other way is it possible to ensure steady improvements in the level of production and therefore in the standard of living, which will be in conformity with the views of freely choosing consumers. The decisions of the authorities whom he proposes to place in charge of the new institutions must therefore be designed in the first instance to ensure the closest possible correspondence between cost and price movements, the latter following upon the former with the utmost possible speed. The price and profit mechanism has, by general consent, worked rather inefficiently in recent years, and especially during the inter-war period, when reductions in costs were often only very imperfectly mirrored in corresponding price movements. The practical problem, however, in Professor Hutt's view, is not to set up new planning machinery which will ignore the price and profit mechanism, but rather to create a new institutional framework within which the forces which have tended more and more to check, if not to disrupt the mechanism, may be kept under control or entirely eliminated. As Professor Cannan taught us thirty years ago, " a man's intelligent pursuit of his own interest generally serves others besides himself simply because the institutions of society provide hedges which are generally close enough to keep him on the road ". In recent years the hedges have been ill-tended and have got into a sad state of disrepair. Some social institutions indeed which have more and more tended to receive the formal approval of the State have had almost exactly the opposite effect to that which Cannan described, ensuring that the profit motive stimulated the restriction rather than the expansion of production, and thus resulting in action harmful to others. Mainly for this reason, " the productive system which Great Britain inherited at the outbreak of war was antiquated and obsolete", and Cannan was perhaps over-optimistic when he added that " when it is found that institutions fail to make it the interest of each man to serve the rest, society abandons or modifies them". Professor Hutt believes that society has been much too slow in abandoning or modifying its institutions, that the necessary institutional reforms are therefore already long overdue, and sets himself the formidable task of providing instruments for destroying the anti-social institutions which have become so powerful, and replacing them by a new institutional framework. The hedges which were suitable in the horse and buggy era are no longer adequate in the days of motor cars (not to mention aeroplanes or tanks); a new type of social road engineer is now required, and Professor Hutt offers him a plan according to which he can construct a new network of social highways, along which the modern vehicle can continue to travel under its own power for the general benefit.

The significance of these considerations is unfortunately still far from being generally appreciated, either by politicians, by business men or by trade union leaders. Private enterprise is still defended or attacked by them as if it were a religion, an end in itself, instead of merely an institutional framework to be judged or amended according to the results which it helps to produce. Many of the self-proclaimed defenders of private enterprise are, whether they realise it or not, in fact defenders of monopolistic restrictionism, and they frequently destroy the foundation upon which their case should rest by putting all the emphasis upon " private", usually interpreted as meaning business privacy, and tending more and more to forget all about " enterprise". The rules in Professor Hutt's plan designed to ensure the steady expansion of production demand for their elaboration and administration wide extensions of State activity. But, as he argues, " neither the rules themselves nor their active execution can be properly regarded as 'State interference'. The rules and the derived State functions do not *interfere with*, they are *part of the institutions of property*".

Professor Hutt's analysis is not confined exclusively to the relatively peaceful conditions of post-war reconstruction. He devotes the first four chapters to a critique of British wartime production policy and to a sketch of the institutional requirements for the efficient prosecution of total war. His criticism of the effects of restrictionist mentality on wartime production has much in common with the views which during the first years of the war found expression in the columns of the *Economist* and the *New Statesman*, from both of which Professor Hutt frequently quotes. In his view, the advantages enjoyed by the Nazi regime in the prosecution of total war arose mainly from the fact that " the right to withhold productive power in the interests of capitalist monopolies and trade unions had been denied by Hitler". " Hitler's strength on the production side has been mainly due to his having kept the cartels and the trade unions in subjection." In the democracies, on the other hand, the pursuit of " legitimate " profits, " fair " wages and security has led to widespread price and wage fixation, which in effect amounted to the same thing as a systematic restriction of output. The Nazi regime has ruthlessly squeezed out monopoly elements in prices, and compelled the closest continuous approximation of prices to costs, so that the right to withhold resources from production has been effectively abolished. " The whole object of price control with our enemy seems to have been to enforce the full passing on to the consumers of the benefits of all cost reductions and the prevention of higher charges for merchanting services."

The most potent force which has checked and rendered ineffective the operation of the price and profit mechanism has been the growing sense of the value of individual economic security. Changes in the structure of production threaten, or appear to threaten, capitalists with the loss of their capital and wage-earners with unemployment. A natural reluctance to submit to such losses has been a fertile breeding ground for all kinds of restrictionist action, the net results of which have been almost uniformly bad. It is however not very profitable merely to inveigh against restrictionism, without taking some positive steps to meet the natural human desire for security, and Professor Hutt accordingly combines with his plan for the continuous and full activity of all productive resources a parallel security plan, which in certain of its aspects makes the Beveridge Report appear a timid and hesitant document.

" Whilst the abolition of price-fixations, output controls, wage-fixations and so forth would enormously increase aggregate dividends and the aggregate earnings of labour, certain conspicuous classes of investors, and certain conspicuous classes of workers, would be faced with ruinous declines in their incomes." As Wicksteed put it, " every one benefits by a good crop in the things he does not grow, but may very well be injured by a good crop of what he does grow " ; people who are urged to abandon restrictionism are naturally a little alarmed lest other people should refuse to play the same game fairly, and some might well take a good deal of persuading before they believed that even if everyone played the game properly they personally would run no risk of serious loss.

How are these fears to be overcome ? Professor Hutt proposes " an income-security directly ensured by the State ". If firm assurances are given that the abandonment of restrictionism will not destroy the reasonable income expectations of producers, the strongest motives for the maintenance of restrictions on production will have been destroyed. It is theoretically conceivable that the distribution of income might be organised in a way completely independent of the organisation of production, and therefore that any system of income-distribution which was thought " fair " could be maintained while at the same time productive resources were employed without hindrance. There is however an important practical limitation to this thesis in the pressure of what Professor Hutt calls " sociological stresses ", the most significant of these being " the weakening of the incentive to productive effort which is likely to follow when an individual's income is made independent of his efforts ". The standard of distributive justice actually accepted is, in Professor Hutt's view, " that established expectations, ' reasonable ' because they are customary, ought not to be violently upset ". Progress in productivity has frequently threatened such violent upsets to the currently accepted distributive scheme, and Professor Hutt suggests a method reconciling the conflict, so that the desire to defend or restore the " reasonable " or the " customary " will not stand in the way of the expansion of production which will be an urgent necessity after the war. " The vastness of the productive task which lies ahead makes it imperative that all the obstacles to untrammelled utilisation shall be somehow overcome." He expresses the diffident fear that his proposal for assuring genuine security in a way which will not damage productivity " may at first give the impression of a Heath Robinson attempt at social engineering ". The publication of the Beveridge Report since his book was written (in 1941) fortunately provides a standard of comparison which should, even to sceptical readers, make his proposal seem more sober than he had thought might be possible. Sir William Beveridge has proposed the levying of a uniform weekly contribution upon all incomes arising from the performance of labour, and the payment of unemployment benefit and retirement pensions at a uniform rate, irrespective of previous earnings. Professor Hutt, on the other hand, proposes that the State should " enact for the individual workers in the different grades of industry and types of occupation (in unionised trades, after consultation with the unions) guaranteed ' reasonable ' minimum income levels ", which should vary according to an authoritative cost of living index. The detailed application of the principle of " established expectations ", to be used in determining these levels, except at the very lowest, is explained with some care. If an individual fails to earn his declared minimum income, he is to be compensated from a uniform percentage levy (at a rate which is not specified), imposed at the source of the earnings of all labour, " the Labour Security Levy," three-quarters of which would be paid into a Labour Security Fund. In addition there should be a National Minimum Income, which would guarantee from the very first a certain lowly security even for the most unfortunate, though where it was necessary to pay the National Minimum, the burden should fall upon general State funds, and only to a very small extent on the funds raised by means of the Labour Security Levy. After the scheme had been established, wage rates should be allowed gradually to rise or fall towards their competitive heights, minimum rates being gradually lowered until say nine-tenths of the workers in any grade were actually earning more than the minimum wage-rate. When that state had been reached, the minimum wage rate (but not the " reasonable " minimum income) would cease to apply, and the valuation of labour be left as free as the valuation of commodities in a free market. The speed with which minimum rates would be lowered would depend on the speed at which the plan as a whole increased the aggregate real earnings of labour, and stringent provision would at the same time be made to ensure that all cost reductions were immediately reflected in price reductions. If the gross earnings of any worker fell below his individual minimum income, they would still be supplemented by a Labour Security Grant from the Labour Security Pool.

Just as Sir William Beveridge proposes that unemployment benefit should " normally be subject to a condition of attendance at a work or training centre after a certain period ", Professor Hutt proposes that the recipient of a Labour Security Grant should be subjected to a discipline exercised by a Board assisted and supervised by Trustees who would be representative of employees, and probably very largely composed of trade union officials. The Board would guide him both in respect of employments accepted and training undergone, all with a view to ensuring that " the guaranteed income does not destroy the motive to seek out the most productive occupations ". He must be prepared to accept orders from the Trustees either to move into a better-paid job or to undergo appropriate training, or else to sacrifice a part of his Security Grant equal to the increased earnings renounced. New entrants to the labour market would be guaranteed gradually increasing incomes on a lower scale, reaching their maximum after ten years of employment, when their established expectations must be regarded as equal, say, to three-quarters of those laid down for existing wage-earners, though their actual incomes would not necessarily or normally be lower. The Labour Security Levy is designed essentially to deal with income variations arising from changes in demand or in the structure of industry ; alongside it, it is urged, there should be a system of liberal, unconditional, compulsory and contributory old age pensions, a liberal health insurance scheme and a liberal workmen's compensation scheme. Unemployment insurance in the ordinary form would disappear altogether, for the unemployed worker would in any event receive at least the National Minimum Income. " The scheme itself provides the most complete and far-reaching system of insurance against fortuitous loss of income that is conceivable." The remaining quarter of the Labour Security Levies would be paid into a Labour Security Reserve, from which would be repaid either on the death of a contributor or at the age of 65 a sum determined by the relationship between his contributions to the Pool and his receipts from it during his working life in the form of Security Grants.

The application of the same principles to salaried and professional incomes, where normally " established expectations " would be a good deal lower than normal actual earnings, creates some special complications, which however Professor Hutt endeavours to show are not insoluble. His detailed analysis of their application to the medical profession, with the consequential or associated reforms needed in the organisation of that profession, is not the least interesting of the subordinate suggestions made in this book.

The provision of security for the recipients of income from property, which is the purpose of Professor Hutt's second bill, is a more novel idea, for which no Beveridge Report is available as a standard of comparison, though there have recently been some similar proposals for compensation for displaced capitalists. Complicated as the problem is, it is, however, unlike the problem of Labour Security, essentially temporary. Capital Security arrangements are required merely whilst the full utilisation of productive power is gradually permitted or enforced. The unfettering of productive capacity will threaten many property owners with the loss of a large fraction of their customary income, or, what may be no less important, it will be believed that it threatens such loss. It is neither desired, nor would it be fair, to expect that there should be no sacrifices, but a reasonable compensation for property owners would be a small price to pay for the large-scale expansions of production in general which are anticipated to follow the downfall of restrictionism. " Ultimately the security of income from capital must be achieved, not by any system of pooling, but by the spreading of investments ", and the small investor can then get the security he needs through the instrumentality of fixed trusts, fortified by stringent auditing and publicity. As a transitional measure, however, a Capital Security Levy, a uniform percentage on all revenues not subject to the Labour Security Levy, should provide a Capital Security Pool from which compensation would be available for such persons as may suffer serious losses in income from defined assets, when such losses are caused directly or indirectly by the enforcement of the Resources Utilisation Protection Act, which is described below, or other legislation administered as part of a post-war reconstruction plan. Such compensation is to be regarded merely as a temporary necessity, confined for the most part to individuals now living. As an insurance against the improbable event of a decline in the aggregate earnings of labour during the life of the Capital Security Pool, provision is also made in that case for the payment of compensation from the Capital Security to the Labour Security Pool.

The possibility of individual ruin having thus been eliminated by the operation of the two Security Pools, it then becomes possible to contemplate seriously the radical reforms needed to set free post-war productive capacity. The real crux of the Hutt Plan is indeed to be found in the *Resources Utilisation Protection Bill* designed to establish an administrative system, under a permanent *Resources Utilisation Commission*, whose primary function it would be " to promote the fullest utilisation of valuable resources in response to the preferences of the community as consumers ". " Its main task will be that of discovering and removing all hindrances to the full utilisation of resources of all kinds, including human resources," " to expose profitableness ", in other words to collect and disseminate the information necessary to decide what production is likely to prove most profitable, because it corresponds most closely to consumers' demands, and to ensure that all the obstacles are removed which have hitherto hindered prompt response to the signals of the price and profit mechanism. Some of the instruments to be used by this Commission are drawn from the experience of the United States, or have already been widely discussed in that country, though there is no slavish imitation of United States models, and its experience is largely used to identify and fill in the gaps which have been revealed in the anti-monopoly laws there. Few British people know much about these institutions, displaying in this connection the same kind of curious parochialism which is now inducing so many to greet as novel " reforms " ideas which were tried out in the early days of the New Deal in the United States, but which are no longer seriously regarded even by the most ardent New Dealers.

The Commission proposed by Professor Hutt would be a quasi-judicial, quasi-administrative body, consisting of nine members, three judges, three economists, and three persons " chosen for their practical knowledge of the institutions of production, trade and finance ", and with a number of subsidiary organs and an adequate staff of economists, statisticians and accountants working under its control. In addition to the suppression of monopolistic practices, the State would also have other vital functions affecting economic activity, for some of which the Commission would be responsible. The State would, for example, be responsible for conservation, the certification of quality, the collective purchase of "risk-bearing through experiment ", the extension and remodelling of the principle of the patent and prevention of the arbitrary use of patents and copyrights, and the enactment of the hours and conditions of labour. The Commission should also have the power to assume the initiative when, in its opinion, a scheme for standardisation, co-ordination or synchronisation is likely to bring material economies, the test in each case being the probability that products or services will be cheapened or made available in greater diversity without any withdrawal of supplies or increase of price of any cheap grades already available.

A considerable scope is also envisaged for state-owned corporations with limited liability, controlled by a State Trading Board, who are to have no privileges, but outbid private enterprise for all essential co-operant resources, including borrowed capital, and with their creditors facing all the risks inherent in credit under limited liability. Not more than one-fifth of the capital in any industry should be allowed to remain in enterprises under State management. The idea of state enterprise as a competitive corrective to private enterprise at one time enjoyed a considerable vogue in countries such as Australia and New Zealand. On the whole the practical results were disappointing, partly because, as Professor Hutt puts it, such state enterprises were " inclined to fall in with the monopolistic leadership set up in the private enterprise sector of an industry ", and to meet this difficulty he sets forth a novel plan for the maintenance of output, too elaborate to be briefly summarised here.

It will be one of the main duties of the Commission to call upon any party to " cease and desist " from stated monopolistic practices which are unlawful under the Act. Professor Hutt believes that the difficulties which have arisen in many countries in connection with the enforcement of anti-monopoly laws are mainly due to the vagueness of the legislative definitions of unlawful practices. Instead of attempting the hopeless task of preventing " unfair competition ", which concentrates attention upon the protection of the competitor, the paramount criterion should in every case be the interests of the consumer, and the Bill " must, without any vagueness or equivocation, make void immediately, and illegal in the future, every contract to restrain utilisation, fix prices, or wage-rates collusively, limit output, restrain trade or exchange, or restrain the acquisition or employment of skill or knowledge (except through patents and copyrights) ", and both labour and agriculture should be specifically covered by these prohibitions. Strikes and lock-outs should be made illegal, because income security and conditions of labour and leisure are guaranteed by other methods. The most effective method for enforcing the legal condemnation of monopoly, it is argued, is through the prohibition, with certain stated exceptions, of price discrimination, the charging of different prices to different consumers. Such a prohibition would incidentally prevent " aggressive selling ", the threat of which is a powerful instrument for warning off interlopers who otherwise might be disposed to encroach upon monopolistic preserves. " Resale price maintenance " practices should also be prohibited, and in general the Commission should have power to investigate and demand any information which it may require about trade and business practices, the relations between firms and like matters, with full and automatic access to all books, documents and records. " Business privacy is incompatible, in a defensible social order, with private enterprise." The proposals for price supervision, made here and in other parts of the plan, have much in common with Miss Joseph's and Mr. Kaldor's recent suggestion that the Government should " fix maximum prices at levels sufficiently low so that only the most efficient firms could survive ".

The Bill moreover endeavours to prevent amalgamations, mergers, holding companies, or the direct or indirect acquisition by a corporation of any of the share capital or debentures of other corporations, while directors and large shareholders are to be personally ineligible to acquire shares in competing corporations. After an interval of ten years no person is to be permitted to be a director of more than one corporation in the same or similar type of production or business, and no shareowner owning more than 5 per cent. of the capital of any corporation will be allowed to retain any shares in a competing corporation, unless voting rights in the shares of all but one corporation are ceded to the Commission. The problem of large corporations already in existence is for the most part dealt with by providing that if there is any unutilised plant or equipment which the Commission believes could be profitably used, it should have power to direct that it should be offered for sale or lease to an independent party.

Lengthy though it is, this imperfect sketch of Professor Hutt's plan has perforce neglected a great deal that is interesting and much that is important. Professor Hutt is well aware of the objections on the grounds of political practicability likely to be raised against a plan many of whose features at first sight seem to run so contrary to many fashionable currents of thought. A more careful examination would however show closer affinities than might appear on the surface between many of Professor Hutt's ideas and those which have been stirring elsewhere and cutting across the old traditional divisions of opinion on economic and political policy. The danger of crystallisation into a neo-corporative state is for the British economy very real and pressing, and no very hopeful results seem likely to emerge from the policy of appeasement which some of the socialists seem disposed to adopt towards the neo-corporatists. Our economic distresses need more radical treatment than this. It is much to be desired not only that Professor Hutt's plan should find the popular exposition by someone with a highly skilful pen, which, as he says, may be an important factor in getting a hearing for new ideas, but also that many of its details should receive an elaborate scrutiny which would run into a volume much longer than the present one. In any event his wide and profound survey of the problem as a whole deserves the most careful attention from all who claim a hearing as advisers on post-war economic policy.

# Miscellaneous Notes

In Professor Hayek's interesting article on "Scientism and the Study of Society", Part II, ECONOMICA, February, 1943, the second footnote on p. 54 states: "It is significant that all of H. Rickert's well-known work on the differences between *Naturwissenschaften* and *Kulturwissenschaften* is based on the assertion that since all phenomena which we can observe are physical phenomena, all generalising (theoretical) science must be physical science". This interpretation is not correct. On the contrary, Rickert maintained that the phenomena we can observe are not exclusively physical and that generalising (theoretical) science includes not only physical science but also psychology. The results of his analysis on this point are summarised as follows: "So ist nachgewiesen, dass sowohl die Ziele der psychologischen Begriffsbildung als auch die Mittel, mit denen sie ihre Zwecke zu erreichen sucht, im allgemeinen dieselben sind wie die, welche wir bei der Betrachtung der Wissenschaften von der Körperwelt kennen gelernt haben, d.h. empirische Allgemeinheit, Bestimmtheit und unbedingte Geltung ihrer Begriffe strebt die Psychologie in demselben Sinne an wie die Körperwissenschaften . . ." (H. Rickert, *Die Grenzen der naturwissenschaftlichen Begriffsbildung*, Chapter II, Section 2, third and fourth edition, page 121).

ALFRED W. STONIER.

Mr. Stonier with his superior knowledge of Rickert's work has indeed caught me in a very careless statement which misrepresents Rickert. What I should have said is that Rickert's work is based on the assertion that physical and mental phenomena present no differences of a kind which would justify differences in the method of their theoretical study, and that therefore all theoretical science must be of the same type as the natural sciences. I would plead, however, though this is no excuse for my hasty statement based on a vague recollection, that Rickert does occasionally express himself as to give the impression my footnote conveys. The passage I had particularly in mind, e.g., runs: "Zwei Gruppen von Objecten, die sich mit Rücksicht auf die Art ihres Seins, d.h. in der Weise wie Körper und Seele von einander unterscheiden, sind für die Gliederung der Wissenschaften garnicht zu finden, weil es, wenigstens in der unmittelbar zugänglichen Welt, nichts giebt, das einer Untersuchung von der formalen Eigenart, wie die Naturwissenschaft sie führt, prinzipiell entzogen werden dürfte. So verstanden ist das Wort, es könne nur *eine* Wissenschaft geben da es nur eine Wirklichkeit gibt, berechtigt." (H. Rickert, *Kulturwissenschaft und Naturwissenschaft*, 2. Aufl., p. 14.)

F. A. HAYEK.

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The London School of Economics has in preparation a new collection of the letters of John Stuart Mill. It hopes to publish in the first instance a complete collection of his letters up to 1847 or 1848. This may be followed later by a further volume or further volumes. For the period till 1847, which in H. S. R. Elliot's *Letters of John Stuart Mill* is represented by only 44 letters, about a hundred unpublished letters and nearly two hundred letters published in other places have already been traced. (For a list of the latter see *The Times Literary Supplement*, February 13th, 1943.) The aim of the work now conducted by the London School of Economics will be to make the new collection as far as possible complete and definitive. The London School of Economics is therefore anxious to get in contact with all owners of letters by John Stuart Mill. Any information assisting in the tracing of such letters belonging to any period of Mill's life will be gratefully received by

THE CHAIRMAN,
Economic Research Division, London School of
Economics. (War-time Address :) The Hostel,
Peterhouse, Cambridge.

# Book Reviews

*The Social Framework. An Introduction to Economics.* By J. R. Hicks. Oxford University Press. 1942. 212 pp. 7s. 6d.

At one time or other almost every Economics Lecturer has felt moved to write an elementary text book on Economics, though not all have succumbed to the temptation. But the number of such books is very large, and most of them follow a more or less stereotyped plan—that is they deal, some more theoretically and others with a wealth of practical illustration, with the problems of production, exchange and distribution. Essentially they are concerned with the theory of value, in so far as they are not purely descriptive.

Professor Hicks has embarked on quite different lines. While throwing out a number of hints that the theory of value is concerned with important and interesting problems, he expressly reserves this for a separate study, and he mainly confines himself in the present book to what he calls " Social Accounting ".

After a short description of the essential nature of the productive process, he deals with two great factors of production, labour and capital, each only in outline, but in such a stimulating and suggestive way as to arouse the lively interest of the reader and make him eager to go deeper into the subject.

The whole of the second half of the book, and this is the most original part, is concerned with the Social Output. It consists of an account, partly analytical and partly statistical (using the figures for this country for 1938), of the income of the whole nation—how it arises, how it is measured and how it is distributed. The analytical argument is worked out with great care and patience, by a step by step process which takes the reader, so to speak, behind the scenes, and enables him to realise for himself how each stage in the reasoning is arrived at. On the statistical side the author has made effective use of the recent official estimates of the British national income for 1938.

The result is a really valuable contribution both to the study and the teaching of Economics, and one which should achieve the hope expressed by the author " that when a beginner has mastered the substance of this book, he will be able to turn to the theory of value with some idea of what he wants to get from it ". Is there another elementary text of economics of which it can be said, as of this one, that it can be read with profit both by the first year student and by the professional economist who spends his life studying and teaching the subject? Moreover, even to that part which is well worn and where countless other feet have trod, the author contrives to bring a freshness of treatment and suggestiveness which is in most welcome contrast to the usual rather uninspiring manual designed for the beginner. This book should fulfil its purpose admirably, and Professor Hicks is to be congratulated on his success in breaking new ground in a field where few would have thought this any longer possible.

C. W. Guillebaud.

*Young Offenders: An Inquiry into Juvenile Delinquency.* By A. M. Carr-Saunders, H. Mannheim, E. C. Rhodes. Cambridge: University Press. 1942. x + 168 pp. 7s. 6d.

Early in January, 1938, at a meeting attended by directors of education, magistrates, chief constables, probation officers and others, it was announced that the Home Secretary desired to institute an investigation into the problem of juvenile crime. As a result of the discussion, it was decided to begin with a systematic research in the London area. Subsequently the inquiry was extended to six provincial cities—Manchester, Leeds, Sheffield, Hull, Nottingham and Cardiff. The general supervision of the work was entrusted to Mr. A. M. Carr-Saunders, Director of the London School of Economics. Two members of the staff of the School have assisted in the analysis of the problem and of the data, namely, Dr. Hermann Mannheim, Lecturer in Criminology, and Dr. E. C. Rhodes, Reader in Statistics. Owing to the outbreak of the war the publication of the results has been delayed; but permission has been obtained to issue the report in book form under the names of the three investigators.

The inquiry now published deals essentially with the social and environmental conditions making for juvenile delinquency. A parallel inquiry into the psychological aspects was originally contemplated, but had to be abandoned on account of the war. The book begins with two admirable chapters by Dr. Mannheim giving a history of previous investigations in this country and summarising what appear to be the main trends in the incidence of delinquency. For the present inquiry the procedure adopted in London was to collect, with the aid of the Probation Office and the Education Department, detailed information regarding the first thousand cases brought before the juvenile courts after 1st October, 1938. The questions to be answered by the field-workers were for the most part arranged in such a form that the answers could be given either by a simple number or by a code letter inserted in the appropriate space upon a record card. Similar information was simultaneously recorded in regard to an equal number of non-delinquents; and much the same procedure was followed in the provincial cities. In all, the inquiry covers nearly 2,000 delinquents, with nearly 2,000 non-delinquents to serve as a control. The inferences are based on a careful statistical comparison of the frequency with which certain conditions have been reported in the delinquent group and in the control group respectively.

In the main, the results confirm the conclusion of earlier investigators who have been limited to smaller groups usually coming from single areas. There is, to begin with, evidence of a well-marked association between delinquency and " broken " families, i.e. families of an abnormal structure. Thus, in the whole series there were 47 homes where the heads of the family were not parents of the child studied and were cohabiting without legal marriage: of these 47 homes, all except four supplied cases of delinquency. Where the family " structure " was normal, it was found that a child living in an abnormal " mental atmosphere " was three or four times as likely to become a delinquent as a child living in a normal mental atmosphere. The chief characteristic of the former group was the excessive harshness or the excessive indulgence of the parents, or an alternation between both extremes. In the homes in which both family structure and mental atmosphere were normal, other suggestive characteristics were discovered. It was found that " the controls had better records of school conduct and school attainment, attended church and Sunday school much more regularly ", and, as judged by their leisure interests, were " a livelier lot of young people " ; on the other hand, the delinquents " appeared to be a somewhat inferior or somewhat subnormal group of lads ". These factors, however, important as they are, do not suffice to explain every case of delinquency. Even a normal boy in a normal home " will occasionally kick over the traces ". " This sort of delinquency," it is added, " has the appearance of being stray or chance " ; and, as the investigators point out, " its frequency is largely governed by the general atmosphere of the time ".

The investigators end by suggesting that many of their findings indicate " lines of action " that can at once be taken with fair hope of achieving some improvement. But above all they stress the need for further research. Such inquiries, they add, should be " not episodic, but regular and systematic ". Any detailed consideration of practical measures must, they consider, await the extension of such inquiries from the social into the psychological aspects of the problem. And, since earlier writers, medical, legal, social, and psychoanalytic, have made so many unsupported statements about the influence of psychological and psychopathological factors, it is to be hoped that competent psychologists will be invited to co-operate. In conclusion, they rightly " utter a warning against expecting that statistics alone can solve the whole of the problem ". What is commonly termed (rather misleadingly) the " clinical " approach—by which is meant, not a medical, but an individual study of each particular case, tracing how the offender's feelings, thoughts, and mental limitations, his motives, conscious and unconscious, and his failures and his successes, in short his whole life-history, have contributed to his lapses—all this is equally essential if we are to interpret aright the precise effects of the various conditions shown to be operative by a merely quantitative survey.

It would be difficult to praise too highly, not only the way in which the general plan has been conceived, but also the care and labour bestowed on the collection and analysis of the data, and the impartial and scientific spirit in which the final conclusions have been formulated. The whole research might well serve as a model to all future investigators of the problem of crime.

CYRIL BURT.

*The Myth of Open Spaces.* By W. D. FORSYTH. Melbourne University Press. 1942. xvii + 226 pp. 17s. 6d.

This book is welcome to students of demography because it contains figures otherwise accessible only with difficulty; but it is also welcome for more important reasons. The author, a former research fellow of the University of Melbourne who spent some time in this country before the war, discusses Australian population problems clearly and concisely with an open mind. Well aware of the criticisms of Australian policy and outlook in these matters, he neither disregards them nor shows any disposition to act as counsel for the defence. Again, we may welcome the book as opportune; it is most useful as a basis for the discussion of a situation which will come under close review in the post-war world. The author claims with some justice in his preface, written in May, 1942, that publication at this time is " in its small way a gesture of confidence in the outcome of the war ", but we could wish that his publishers had displayed equal confidence in the desire of the public to buy a good book if priced within its means.

The book falls into two parts, of which the first is called " New Magnets of Migration " and the second " The End of British Migration ". In the first the author is concerned with the general phenomenon of the concentration of population and the drift to the cities, and with the special relation of Australia to these matters. Space, as he says, no longer means opportunity; for it is the primary industries which are saturated. Western civilisation has now little use for the " vast empty spaces " with their " unlimited resources " which were so much advertised in the past and which still haunt the minds of some publicists and politicians; opportunity to-day resides in areas of concentrated population provided that the doors to trade remain open. Moreover, the climate, soil and configuration of Australia are such that the existing " empty spaces ", though ' vast ', would be ill suited to settlement even if world trends were other than they are. Little can be anticipated from closer settlement. On these and allied topics Mr. Forsyth has much that is informative to say. It follows that in the future opportunity in overseas countries, and especially in Australia, must be sought in industrial development. Here Australia has not been backward; only England, Switzerland and Holland, for instance, have a smaller proportion of their people engaged in primary production than has Australia. The release of workers from the land consequent upon the mechanisation of farming means that for a time and up to a point industrial recruitment can be supplied from this source. But the industrial population ought to grow steadily for a long time ahead, and the question is whence are the recruits to come; for the prospects of industrial development in Australia are " fair " but not " spectacular " in our author's view.

This leads to an examination of the prospects of Australian population growth by way of migration and natural increase. From what quarter may immigrants be expected? Not from Northern or Western Europe; a summary of the well-known prospects in this region is enough to show that the countries in question are more likely to attract than send out immigrants in future. Recruitment might come from South and East Europe, at least for so long as this area displays its present fertility and shows evidence of congestion. On this subject Mr. Forsyth has much to say that is interesting. Recently Australia has been experiencing the " new immigration " that became well known to the United States about the turn of the century; up to a few years before the war alien immigration from this region was a small fraction of a large immigration; more lately it has been almost the whole of a small immigration. " There is very little evidence of friction retarding the assimilation of these people into the Australian economy and, on the whole, into the Australian way of life." These easterners and southerners are widely scattered over the continent and do not form enclaves. " The alien on the whole makes a good citizen, works hard and, if left to himself, does not trouble much about politics." (Mr. Forsyth is presumably thinking of " politics " in the form of totalitarian propaganda from overseas; otherwise this statement would hardly be a commendation of Australian politics.) But South and East Europe are going the way of North and West Europe, and Australia cannot long rely upon immigration from this source.

What then about Asiatic immigration? About this matter Mr. Forsyth thinks that Australian policy is fixed; a white Australia, though not perhaps a British Australia in the sense that all Australians should trace their ancestry to residents in this country, remains the goal. This brings our author to conclude that Australia, like other countries of western civilisation, must attend to its own fertility. For in a final chapter Mr. Forsyth shows that in his opinion a " teeming " Asia and an " empty " Australia cannot continue to exist indefinitely side by side. " For the Dominion to ignore the trends of population in Asia, the problems they create, the rising temper of Asiatic nationalism and the growing determination of Asiatic people to improve their conditions and status would be the worst possible blindness and stupidity. It should be a prime point in Australian policy to press for a radical solution of the problems of overcrowded Asiatic lands. If we won't have these people coming here to live, well, we must help them to live well in Asia."

A. M. Carr-Saunders.

*Unused Resources and Economic Waste.* By David Rockefeller. University of Chicago Press. 1941. x + 260 pp. $2.

Dr. Rockefeller's commendable thesis discusses the notion of " waste " in unused *physical* resources, in so far as such resources are " consciously invested in and maintained at cost " (p. 18). Thus, he attempts to exclude from consideration (i) human resources and (ii) natural resources, although they are surely equally liable to " waste ". And on the demand side, he tries to exclude from consideration certain types of so-called " non-economic " consumption. That is, the demand complex which determines the content of *productivity*, which must in turn be the criterion of *waste*, is also limited. But if the pursuit of certain so-called " non-economic " or " non-pecuniary " ends (e.g. any collective social ideal) involves the sacrifice of pecuniary ends, then the economic aspect is surely just as relevant to these supposedly non-economic ends or ideals as it is to any obviously pecuniary end. For instance, if a policy of " vigorous Government spending ", to produce relatively non-competing (i.e. relatively undemanded or unproductive) capital assets is adopted in order to avoid the distributive consequences of full productivity, then the resultant income stability must be regarded as an economic end achieved. The stability is itself a product, involving a collective cost in the sacrifice of goods and services demanded through the market. That is, it is a collectively demanded thing and so not truly " unproductive ". We feel therefore that the limitations which the author has imposed have hindered rather than aided his study. A more complete generalisation would have brought greater clarity.

In his treatment of waste as affected by changing conditions under the business cycle (Chapter IV) Dr. Rockefeller's exposition is weakened by his failure to apply sufficiently rigorously his assumption of " purely competitive conditions ". It has certainly never been proved that, under " competitive conditions ", a general fall of prices would lead to a contraction in the *range* of valuable resources. That is, *in the absence of withheld capacity*, there is no reason to suppose that resources will be used less intensively as the book under review assumes (e.g. pp. 85–86). At times, it is clearly the oligopolistic situation rather than the competitive which he is discussing, especially when we are told that " fears of ' spoiling the market ' and of ' cut-throat competition ' " . . . add to the entrepreneur's " reluctance to face the new situation through readjustments in his price policy " (p. 84).

The weaknesses of the book are, we feel, mainly due to the tools which Dr. Rockefeller has chosen to employ. That is, the above criticisms are aimed more at the accumulated body of method and concepts[1] which his training has placed at his disposal than at his own contribution. A closer understanding of Wicksteed's and Robbins's approach, particularly the latter's chapter on "The Relativity of Economic Quantities" in *The Nature and Significance of Economic Science*, would have been of inestimable value to him.

But these criticisms must not be allowed to detract unduly from the real value of the contribution which Dr. Rockefeller has made. He has most acutely criticised some of the recent attempts to study the origin and significance of idle capacity, and he has been especially devastating in his treatment (in Chapter VIII) of three attempts to treat the problem empirically; he has usefully re-stated the relevance of entrepreneurial foresight to the provision of specialised resources; he has commented shrewdly on the relevance to waste and idleness of modern institutions like corporations, employer and labour organisations, financial institutions, business customs, social legislation, taxation, and anti-trust legislation; and he has caused the crystallisation of many most important issues of policy. The ultimate implications of the book have a direct bearing upon the problems of reconstruction everywhere, and the student who wishes to devise practical methods of achieving full employment in all resources will find this distinguished thesis suggestive and stimulating.

W. H. HUTT.

*Studies in Mathematical Economics and Econometrics.* In Memory of Henry Schultz. Edited by OSCAR LANGE, FRANCIS MCINTYRE, THEODORE O. YNTEMA. The University of Chicago Press, Chicago, Ill. 1942.

"The search for truth is in one way hard and in another way easy, for it is evident that no one can master it fully or miss it wholly. But each adds a little to our knowledge of Nature and from all the facts assembled there arises a certain grandeur." Aristotle's wonderful words seemed a particularly fitting conclusion to the monumental labours of the *Theory and Measurement of Demand*. A certain grandeur arose out of the patient analysis of a large variety of facts, the use of economic analysis to illumine statistical method and the use of statistical method to give body to the generalisations of economic theory. It is difficult to do justice to that great quality of solidity and unity of purpose which distinguishes Schultz's work in a memorial volume completed in war-time which of necessity contains work of heterogeneous quality directed to varying purposes with unequal success.

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[1] The reviewer discusses these concepts in a forthcoming article in the *South African Journal of Economics*, to be entitled "The Concept of Waste".

The volume consists of four parts. The first part contains three personal appreciations by G. K. K. Link, Theodore O. Yntema, and René Roy, and a list of Schultz's publications which should prove very helpful to all students of econometrics. The second part contains essays in pure economic theory by O. Lange, Paul A. Samuelson, J. L. Mosak, G. Tintner, A. G. Hart, M. Bronfenbrenner, W. Jaffé. The third part is devoted to statistical theory and contains articles by J. Marschak, J. H. Smith, H. P. Hartkemeier. The fourth part contains works in econometrics by W. A. Wallis and M. Friedman, R. G. D. Allen, R. H. Whitman, Joel Dean, J. Tinbergen and Charles F. Roos.

It will perhaps not be expected, even of a reviewer, that he should be uniformly stimulated by everything that is presented. Two of the articles in Section III I frankly admit I did not read with that care which would be essential to form a reasoned judgment on them. Much the most interesting contribution I found to be Marschak's article on economic dependence and statistical analysis. It shows the sort of interpenetration of economic ideas and statistical analysis which would have delighted Schultz. The analysis here presented has been developed further in a brilliant article by T. Haavelmo (*Econometrica*, January, 1943), to whose help in writing his own article Dr. Marschak pays a generous tribute. The chief argument of Dr. Marschak's contribution runs as follows: Hitherto macrodynamic systems have been subjected to five stages of analysis. 1. A system of economically plausible equations—the theory to be tested—is set up. The number of equations must equal the number of unknowns. At this stage the parameters of our equations can be left undetermined. 2. For the equations other than identities the numerical values of the parameters are estimated either by means of an analysis of time series or by means of an analysis of budget date or by means of an analysis of variation between different localities. 3. By means of a process of elimination a functional equation in one variable is obtained. 4. The functional equation is solved and its arbitrary constants are determined in terms of the initial conditions. 5. The expected time series is compared with the observed time series.

Dr. Marschak suggests that the process of elimination should be carried on *in abstracto*—the values of the parameters being left undetermined. At this stage—and only after the final functional equation is obtained—should the fitting process take place. This suggestion is extremely plausible on intuitive grounds, but the argument given by Dr. Marschak against the old procedure, namely, that it incurs the risk of random errors accumulating in the process of elimination, is not entirely sufficient. If that were the only risk, many statisticians would with reason prefer the old procedure, for there is a great advantage in being able exactly to state what economic interpretation we are going to put on a given numerical result. The process of elimination yields parameters which are a hotch-potch of the original parameters and we are thus faced with the difficulty of not being able to bring to bear on our numerical results such qualitative information and common-sense as we may happen to possess. The final argument against the old procedure is that it leads to biased results, not merely to large standard errors in the fitted constants (*Cf.* Haavelmo's article). What conclusions ought we to draw from the failure of the old procedure ? Must we really fit our constants only after we have carried through the elimination process ? That seems to be Dr. Marschak's conclusion. It may be pointed out, however, that what he has proved is merely the incompatibility of the assumption of interdependence and of the old statistical procedure. Another way out is therefore open. We might try to determine our constants under conditions where this interdependence is negligibly small. To take an example : it may be very difficult to disentangle the " incentive to invest " function from the " propensity to consume " function if we analyse time series referring to the economy as a whole. Nevertheless for smaller units, either localities or industries, it should be possible to determine the " incentive to invest " function without ambiguity. Thus, for the purpose of determining our parameters, we split up the interdependent system into a sufficient number of independent systems. The procedure has its dangers. Within any partial system the rate of investment depends very much on the " reserve position " of entrepreneurs. Within the total system this dependence almost disappears. What is true of the investment function is true *mutatis mutandis* of other functions. In short, we must be clear that in summing over a large number of partial systems certain factors which loom large in any given partial system may be ironed out whereas other features which are barely distinct in the partial system acquire great importance. Nevertheless, I believe that very frequently the fitting of simple equations within partial systems will be preferred to the fitting of very complex equations within the total system.

I should next like to comment very briefly on three articles in Part II of the book. Mr. Mosak shows that " the fundamental equations of the theory of value can be interpreted in two different ways. We can think of the ' substitution effect ' as that change in consumption in response to a given price change which would take place if a consumer were compensated for his loss in real income. Alternatively we can think of it as the change that would take place if a consumer were compensated for his ' apparent loss ' in income, the ' apparent loss ' being the loss sustained if a consumer continued buying exactly the same budget after the price change as before. The first substitution effect is Professor Hicks's, the second Slutsky's. Both are mathematically represented by the same formula and it follows that the difference between them is of the second order of smalls, a result which is of great interest in connection with index-number theory. Mr. Paul Samuelson in a very interesting paper on the constancy of the marginal utility of income shows that strictly interpreted the constancy of the marginal utility of income with respect to price leads to a linear Engel curve with unit income elasticity of demand. I should like to append one minor criticism. Perhaps the author might have pointed out more explicitly that all the important conclusions drawn from the constancy of the marginal utility of income can be drawn from the assumption that a small proportion of income is spent on any given commodity.

Professor Lange restates and reconsiders Say's law. It states that whatever the level of money prices and whatever the system of interest rates the total supply of commodities and services always equals the total demand for commodities and services. This implies that a universal glut of all commodities and services, a universal profit deflation, is impossible. Money being only a means of exchange, not a store of value, is held quite independently of the general price level or the system of interest rates. Professor Lange shows very clearly that Say's law is essentially incompatible with any theory of money. In particular, since the total demand and the total supply of commodities and services are equal quite independently of the level of money prices, we cannot say that the general price level is such that the demand to hold cash just equals the supply of cash.

Finally I should like to mention Mr. R. G. D. Allen's study of the expenditure patterns of American families of different types. One very striking result is that there seems to be no well defined difference between the structure of expenditure of urban manual workers' families and urban clerical workers, at least not so long as families of moderate size are compared. As the size of the family increases differences become apparent. I suspect that the correlation between family size and the difference between working class and clerical expenditure pattern is " symptomatic ". Those working class families that have become assimilated to a middle class way of life have small families. It is possible too that working class families of protestant communities show a greater tendency both towards acquiring a middle class standard of life and towards family limitation. A further interesting result of Mr. Allen's study is that the effect of family size cannot usually be allowed for by simply " calculating equivalent adults " for each type of expenditure. The reasons appear to be that the equivalence relations vary with the size of income and the number of the members of the family, and that variations in family composition affect the family taste significantly. Unfortunately this analysis is not pushed further.

E. M. Rothbarth.

*The British Tariff Movement.* By Marvin E. Lowe. American Council on Public Affairs, Washington. 1942. 133 pp. Cloth, $2.50; paper, $2.00.

The whole question of international commercial relations is obviously of first-class importance from the point of view of post-war economic reorganisation, and it is essential that the reasons for and trend of public opinion in each country on this subject should be understood by the other members of the United Nations.

Mr. Lowe has written a clear and readable account of the revival and victory of protection in England, from the early years of this century until the passage of the Import Duties Act of 1932. His main object is to explain the events leading up to this fundamental change in policy, with special reference to the " beliefs, attitudes and pressures of particular interests, groups and leaders ". On the whole he succeeds in presenting a well-balanced and illuminating picture of the immediate background of the revolution in tariff policy, eminently suited to explain the situation to the American reader. On the other hand, he deliberately confines himself to the history of opinions and public controversies on the tariff question in the narrow sense, excluding any discussion of theoretical considerations, and all but the most unavoidable incidental references to allied controversies and problems—such as the Agricultural, Currency and Balance of Trade problems—with the result that he frequently fails to do full justice either to the actual problems involved or to the arguments advanced. This is particularly noticeable in Chapter VI, where he deals with the most critical period of all, and where only a somewhat garbled account is given of Keynes's attitude and arguments. The American reader would be well advised to supplement this history of public controversy by reference, for instance, to Dr. Benham's *Britain Under Protection*, where both the actual tariff policy pursued, and allied subjects (such as agricultural policy, import quotas, subsidies and imperial economic relations) are analysed in much more detail.

In several ways the most interesting section is that dealing with the repudiation of free trade by the left-wing labourites. It is easy to forget the variegated ancestry of the demand for import boards, and to overlook the dangers arising out of the confused and sometimes contradictory arguments and motives underlying this demand. It is also interesting to be reminded of the great similarity between Mr. E. F. Wise's schemes for international commodity control and the International Wheat Agreement of 1942, although Mr. Lowe himself does not refer to the latter, which was presumably issued after he had completed his book.

VERA ANSTEY.

*The Economic History of Steel-Making, 1867–1939*. By W. L. BURN. Cambridge University Press. 1940. x + 548 pp. 27s. 6d.

The late Professor Knowles, it is reported, used to say that for her purposes economic history ended at 1886. This choice of terminal date was a little odd. It is true that a Royal Commission on the so-called " great " depression of trade reported in that year; and though by then it had run only a half of its quarter-century span, it is tempting to wind up one's history with a seductive bit of finality in the way of documentation. Yet that hardly seems reason enough. Why not have employed for that purpose the equally elaborate, though less valuable, report of the Royal Commission on Labour half a decade later? It was presumably too early then, as it often seems to be still, for the problems of labour to appear as more than economic pathology. It is not surprising, therefore, that this second highly elaborated, and let it be admitted very far from satisfying, Report did not lead Mrs. Knowles another five years on. When she was busy, twenty years back, studying and writing in order to illuminate and to integrate the development of the industrialised countries, and their relationships with their backward dependencies, the year 1886 was as good as any other to choose for the beginning of the years which it was then customary to regard as a no-man's land of shadows between the distinct disciplines of the historian and the economist. Now things are different. Mr. Burn brings his study up to the date on which he lays down his pen. His work is informed by the curiosities of the economist—he asks economists' questions of his materials rather than historians', if it is permissible to put it that way. He even defies the conventionalities by starting with the puzzlements aroused in the minds of industrialists and politicians by the comparative efficiency or the appearance of it, of our rival foreign manufactures of steel and steel products rather than with the great technical advance announced by Bessemer in 1856. Indeed, the technological history between 1856 and 1867, and the technical processes employed in the opening years of the period studied, are embedded in text or appendix in their appropriate, and in some degree incidental, setting rather than unfolded systematically. Mr. Burn sub-titles his work *A Study in Competition* and he does not concentrate, therefore, on the history of steel technology in his period. For this students of economic history, and of economics, may well be grateful. The Transactions of the Newcomen Society, the technical journals of the industries concerned, and the applied scientists' treatises based on them are not economic history, important though they are to its study. It is good to find that Mr. Burn writes convincingly on the enlargement of technique, but it is still better to find that his book is free of pictures and blue-prints of the converters and what-nots whose adventurous past is the important prime interest of other specialists rather than of the economic historian. Mr. Burn's interest in what went on inside the plant of his producing units is lively and adequate, but it is not his prime interest. He is concerned with markets, and what went to their making and unmaking—costs both of capital and labour, the structure of prices, the play of tariffs, the forms of organisation and the determinants of public and business policies. His book is economic history, not technological or social history, and as such it fills most adequately a serious gap in our literature. It is distinguished by its thoroughness, its cool sobriety of judgment and its undeviating determination to get beneath the exaggerations, distortions and omissions of contemporary opinions and statistics to the underlying realities of the phenomena he is analysing. Mr. Burn never attempts the dramatic or the exciting in his writing, and sometimes one wishes he would brighten it a little, but for the workmanlike character and value of his finished product—on which he spent several years of devoted labour—there can be nothing but praise. If similar studies of coal, shipbuilding and other 'basic' industries could be made, on the model of this book, not only would dark patches disappear from the manuals of 'recent' economic history, but valuable experience for the policy-makers would be rendered available in this our difficult period of semi-planning.

Mr. Burn opens his history at the Paris Exhibition of 1867. By then Bessemer and Siemens had made possible the large-scale manufacture of cheap steel, at this stage the basic raw material of expansion in communications as well as of the arts of manufacture and destruction. In fuel economy foreign manufacturers of steel were already ahead of their English rivals, and the question of costs was at the heart of the controversies in which the steel industry, more than most, was plunged by those who voiced the recurrent fear of England's economic decline. There was much economic introspection, much discussion of industrial training, and when in 1878 onwards tariffs began once more to mount, much argument about the various factors in financial as well as technical efficiency. Mr. Burn divides his history into three phases—a first phase of mid-Victorian Alarms (1867–1878), a second that leads to the Early Edwardian Climax (1879–1904), and a third of War and Peace (1905–1939). His method is to outline in each period the historical development of the industry and then in separate chapters to analyse the factors of progress or stress or location or technique, whether these be internal or external or both. The first of these phases is relatively lightly treated. A possible complaint is that Mr. Burn does not give more space to the early history of Bessemer production, and to the history generally of steel production between 1867 (or 1856) and 1878. It is not unimportant that the behaviour of our steel masters in this first expansive phase should be known—Bessemer's calling in of his patents because they were not used; the continental and American war-markets; the problems of costs before the onset of depression in 1873. There are many retrospective glances, of course, so that these and related experiences are not passed over in silence. Fuller treatment would have enabled the student to modify some of the confident generalisations with which early Victorian free-trade history is studied and would have made some of the later depression experience appear less open to wholesale criticism. The history of industries, virtually new as steel was in the post-Bessemer phase, in their first phase of rapid expansion when new techniques have become available is illuminating in all sorts of ways, and the problems of their capitalisation are particularly so. However, Mr. Burn's text is over 500 pages as it is, so that no neglect can be alleged for his somewhat late start, and its " in-mid-air " character. Where criticism may emerge is in his distribution of emphasis upon the various factors which are responsible for the relatively slower advance with the new technique than that of the new rivals. The English employers suffered from their narrow attitude both to the necessity of technical education and of adequately controlled manufacturing costs. They suffered from the disadvantages as well as the advantages of pioneering, that is, and did not fully realise how circumstances such as the contemporary predilection for armaments and railways worked rather to the benefit of their competitors than of themselves—or, at any rate, that these circumstances in conjunction with their own excessive individualism did. Much turned on the question of labour costs, and it is doubtful if Mr. Burn, even in his admirable chapter on the economy of low wages in his middle period, has said the last word on it. It is hard to remember that wage-disparities are not due to original sin in present and past generations of wage-earners. Mr. Burn never descends to superficialities, but he could have given even more weight than he does to a common opinion even among British steelmakers that high wages were less of a manufacturer's burden than polemical utterances led many to think. And in this industry the maintenance of industrial peace has been easier than in many industries—the trade unions' services, that is, have been helpful to employers as well as to employed, and have done much to set-off disadvantages that may have arisen from wage-costs higher than those of Belgium, though lower than those of U.S.A.

Mr. Burn has discussed very fully the problems of concentration in the steel industry. Some critics have said of his work that he allows less than he should for such factors as the British aptitude for work of high technical accomplishment and maximised durability. This criticism may contain an element of truth, but it is not one that the present reviewer shares. So careful and impartial is the discussion both of the history of the structural adaptation of the industry to conditions before 1914 and of the meaning to the industry of foreign tariffs and of Mr. Joseph Chamberlain's English ' tariff-reform ' campaign that it is hard to believe that Mr. Burn has missed anything. Mr. Burn comes down on the side of reorganisation rather than of tariffs, but a statement in those terms does less than justice to his analysis. He makes no claim for protection pure and simple, but he is willing to consider the tariff as adjunct and condition of reconstruction, and it is with this problem that his interesting last chapter is concerned. In fact, it makes a useful final commendation of this excellent book to say that his very restrained discussion of the effects of the last war on the steel industry deserves a very careful reading. Experience since 1939 adds its own urgency to this discussion. Those who wish to wrestle in advance with the problems of reconstruction after the present war will gain much from the integration of Mr. Burn's analytical narrative with our own wartime difficulties. They will appreciate Mr. Burn's restraint and his judicious weighing of the evidence. They will also be a little impatient with its limitations —perhaps, too, with those who provided it, and even with Mr. Burn's refusal himself to be impatient every now and then. At some point or other, the politicians follow upon Mr. Burn's tracks. They have a Four-Year-Plan upon their hands now, with State encouragement of export markets and goodness only knows what else besides. Mr. Burn has not made a single caustic remark about the steel magnates who managed this basic industry for us: everybody else who has studied this industry has! He ought now to write an anonymous study in which he deals with the industry in the light of experience subsequent to that which he details. He knows it well and it would be very useful. If he doesn't, perhaps it will be "planned" out of "private" enterprise, or be given too free a hand, or too much protection. . . . These big issues arise from Mr. Burn's pages quite naturally. The position outlined in the closing chapters has gone now. It would be good to know what so discerning a guide as Mr. Burn has to say of the future.

H. L. Beales.

*French Predecessors of Malthus.* By Joseph J. Spengler. Duke University Press. 1942. 398 pp. $4.50.

Professor Spengler is one of those who, in a familiar phrase, have *furchtbar viel gelesen.* He has already written on population questions, with special reference to France, and in his previous work he felt the lack of a detailed study of French views during the 18th century on population and wage questions. No subsequent student in this field of enquiry will have any occasion to regret the absence of such an aid to his researches; for in the preparation of this volume Professor Spengler has dredged France of the 18th century so effectively that the reader may feel, with justification, that nothing has escaped the net. For the same reason, this will prove for most (if not all) readers a chastening book, convicting them of ignorance which may or may not be excusable. There were apparently so many writers in the 18th century; and, if we be honest with ourselves, we shall admit that with most of them we have but a nodding acquaintance. It may indeed be that there are quite a number to whom we have not yet been formally introduced. When we are told, for instance, that "Mid-eighteenth-century populationism is best represented in the works of Plumard de Danguel, Goyon de la Plombaine, and Jaubert", how many of us are qualified to engage with Professor Spengler in a knowledgeable discussion of the question?

Moreover, Professor Spengler has interpreted his terms of reference widely. If wages signify subsistence, then obviously wages and population are kindred subjects; but if the door is opened to wages, the whole theory of distribution, and for that matter the whole of social and economic theory knocks at the door and claims the right of entrance. While Professor Spengler has therefore in the main restricted himself to views on population, there are some whom he discusses who had little to say on population questions, as strictly interpreted; and again, lured on by the attractiveness of others, he writes at times as though he were concerned with a general history of the development of French economic thought.

It may not be possible to do more than indicate the general framework of Professor Spengler's discussion. An opening chapter throws a backward glance to the mediævalists and the mercantilists. Already in Montchrétien we have a reference to the 'infinite number of inhabitants' whom France might support. It is curious how frequently in one school of thought one encounters this unguarded expression. Probably they did not have in earlier times that feeling of mathematical respect for 'infinity' which now comes naturally to us. More cogently it may be suggested that they commonly regarded Land and Labour as the two agents of production, and those who emphasised the greater importance of labour found no difficulty in conceiving the possibility of labour supporting labour to an indefinite extent. In the first chapter we encounter great names: Colbert, who was practically concerned to increase population; Boisguillebert, who, more than most, deserves to be better known; and Fénelon, who in his pleasant Utopia of La Bétique and Salentin, was even more muddled on population questions than these pages reveal. Perhaps rather oddly, the writer who figures as prominently as any in this section is Belesbat who, so far as we know, has not hitherto been advanced beyond a becoming obscurity.

Thereafter in seven chapters Professor Spengler presents the 18th century, endeavouring to group his writers according to their tendency. The impression one gets, however, is that they refuse to be grouped; and despite their segregation, his writers frequently call to each other from their separate compartments. Doubtless the chronological factor is to some extent a useful subsidiary aid to separation. The second chapter is devoted to the 'Neomercantilists and the Agrarians', concerned with restoring prosperity. The most interesting here is Ange Goudar (1720–1791), who gives such an overwhelming list of checks on population that the timorous reader may well wonder how he comes to be alive. He also provides an astonishing and rather puerile list of encouragements to population, which reveals this line of thought at its most fatuous, e.g. that holders of annual offices should not be replaced by persons with fewer children, and that in deliberative assemblies members should speak in the order of the number of their children—a suggestion which would have some curious repercussions on the House of Commons. The third chapter is devoted to the "Repopulationists", inspired by the prevailing belief that the population of France was declining. Their argument resolves itself in large measure into an attack on celibacy, and lamentations over the decline of *mœurs*. There are actually some who deem it "more seemly to have six horses in the stable than to supply the State with children and live in mediocrity" (Danguel). It is curious how the motif of 'horses *versus* men' runs throughout the 18th century. There is a familiar ring about the complaint of the Chevalier de Cerfvol that "it enters the plan of some households to have no children or only one". Here again, in their attack on celibacy, there is ample evidence that Goudar had no monopoly in puerilities. Poncet de la Grave held that Christianity was unfavourable to multiplication, as its principal object was "to people heaven"; and among his remedies is a suggestion that celibates should be compelled to attend public assemblies, in order that they might be sent away with the crushing rebuke: 'Retirez vous, comme indignes d'être membres de la société.'

The following chapter is devoted to the general theory of Cantillon and the curious place he assigns to the behaviour of the landed classes in determining the shape and structure of society. Here also we meet writers primarily concerned with *Luxe*. It is significant that Mirabeau is one of those who hanker after an infinity of men, and he also, with much emphasis, plays on the 'Men *v.* Horses' theme: 'If you place one horse more in the State, other things remaining equal, you are certain to kill at least four men.' The fifth chapter brings us to a general discussion of the Physiocrats with, inevitably, Quesnay in the leading place. In the main their contention was that revenue was more important than population, which was a result rather than a cause. Thereafter in a chapter devoted to 'Philosophes' we mingle with the great, and some not so great. Montesquieu is chiefly interesting because of his firmly rooted conviction that the population of France, and of the world, had declined. Equally odd, and equally indefensible, was Buffon's belief that the population of the world was an invariable constant: "Whatever care man may bestow on his own species, he will never render it more numerous in one place, but at the expense of an equal diminution in another." Here also we have the optimists with faith in progress, Condorcet being the most notable, but the Abbé de Saint-Pierre also claiming some significance for his declaration that there could be no danger of over-population for 30,000 or 40,000 years: and, needless to say, much may happen in that time.

The chapter on the so-called Non-physiocratic economists also contains a mixed collection of names, of which the most distinguished is Turgot. He deserves notice as the first to give a clear statement of the Law of Diminishing Returns. One reason why the 18th century thinkers on population floundered as they did is that their equipment on this point was so hazy and inadequate. The most significant section in this chapter is, however, that dealing with mendicancy and the Report of the Comité de Mendicité, an illuminating document, both with regard to the conditions and the thought of the times. Lastly, apart from a chapter in which Professor Spengler speaks with his own voice as commentator, there is a chapter dealing with the " Extreme Antiphysiocrats ". The title is perhaps misleading. Those who figure here are the precursors of Socialism—Necker, Linguet, Mably and Morelly. The discussion of Necker is of interest and importance; but though Mably and Morelly are significant in history (if only as the connecting link between Plato and Babeuf), they are not really of much significance on population questions.

Professor Spengler has written an interesting book; but if it be not an ungrateful observation, one is left with the impression that it is not so interesting as it might have been, or as it ought to have been, having regard to the enormous amount of work the author has put into it. Nor is the reason for this slight sense of disappointment difficult to find. It is that Professor Spengler has overcrowded his gallery; and at the same time he has kept himself too much in the background. The number of writers who are " presented " in this book is enormous; if truth be told, a very great many of them say the same thing. There is in consequence a very considerable measure of repetition, accentuated by the fact that the various chapters do not really separate off the writers. The discussion on *Luxe* is everywhere; so are enumerations of the checks on population, with a strong family resemblance between the various lists. The consequence is that this is rather a wearisome book to read consecutively from page 1 to page 386. On the other hand, Professor Spengler has summarised most admirably the writings of his authors. If anyone wishes to know just exactly what Forbonnais, or Auxiron or Du Buat-Nançay said, he will find it most excellently reproduced here. And if we could have spared some of the smaller fry, e.g. Graslin, of whom we are told that he 'virtually ignored the population question', so we could have done with more of Professor Spengler himself. Substantially, if not entirely, he confines himself to the last chapter on 'Conclusions and Interpretations': but indeed, in threading our way through this crowd, we require an interpreter and guide as a constant companion.

ALEXANDER GRAY.

*Infant and Maternal Mortality in Relation to Size of Family and Rapidity of Breeding.* A Study in Human Responsibility. By C. M. BURNS, M.A., Department of Physiology, University of Durham. viii + 247 pp. 5s.

Mrs. Burns has set out on an investigation of a subject which is of great interest and importance to the sociologist, the human biologist and the demographer alike. She has tried to disentangle, as far as possible, the various factors, social as well as biological, which make for infant and maternal mortality in this country. Her material consists mainly of the record cards of the Maternity and Child Welfare Department of Durham county, and she has investigated the subsequent history of 17,049 children who were born there in 1930.

With the need for conserving as many infant and maternal lives as possible becoming more and more urgent as the fertility rate in this country decreases, it is important to find out how far the annual waste that infant and maternal mortality represent is preventable. Certain writers have taken the view that the high infant death rate is primarily a biological phenomenon; the late Professor Pearl is quoted in this report to have said: "The death rate of the earliest period of life is selective—eliminating the weak and leaving the strong." On the other hand, regional, social, and international differences in these mortality rates have led some writers to suppose that a reduction may be expected from an amelioration of social conditions, and they have cited the spectacular fall in the British infant death rate in support of their conclusion. If, however, an association exists between the age of the mother, the parity order, and the rapidity of breeding on the one hand, and infant and maternal death rates on the other, part of the decline at least may have been due to changes in these factors.

As social factors may operate in addition to the biological ones already mentioned, the statistical complexity of the problem becomes evident, and Mrs. Burns does not always succeed in giving a clear cut answer to the questions she is asking. Her treatment of nutrition and housing especially is rather cursory in character, and some of her conclusions there are at variance with those of other writers in the same field. In part, however, the difficulties are due to the need for breaking up the data into a large number of separate sub-groups, so that the death rates in these groups are based on a small number of cases and are therefore subject to large errors. Though the author mentions tests of significance in passing on p. 31, they are rarely used in the body of the report, and some of the results might benefit from the application of more refined statistical analysis.

The treatment of the factor "rapidity of breeding" more especially seems to me to be not very satisfactory. There is little doubt that it exercises a great deal of influence on infant and maternal mortality, but, surely, the right measure to use is the mean interval between successive births. Mrs. Burns only considers the rapidity of breeding for families with a fairly large number of children. It would be interesting to know, for instance, whether the mortality of second children is appreciably affected by the interval separating their birth from that of the first child, or whether rapidity of breeding only has deleterious effects when it is also of long duration.

The main merit of this work consists in the presentation of a number of interesting data, and, incidentally, in demonstrating how much important material is available in local records. The nature of the problem, however, makes small scale local enquiries of little value, and one would like to see whether the results would be corroborated by similar enquiries in other areas. Until that is done the author's conclusions must be treated with caution.

In conclusion, there is a personal complaint I would like to make against a deplorable practice adopted in this report, which is unfortunately becoming all too general. I refer to the segregation of statistical tables and footnotes to the end of individual chapters, even though they are essential to the understanding of the text. It cannot be too strongly emphasised that the place for these is in the text, and one reader, at least, finds it a source of perpetual irritation and annoyance to be compelled constantly to have to refer to two or three different pages at once.

E. GREBENIK.

*Indian Provincial Finance, 1919–39.* With special reference to the United Provinces and with an additional chapter on Provincial Finance under the 1935 Constitution. By B. R. MISRA. London: Oxford University Press. 1942. xvi + 349 pp. 11s 6d.

This is an interesting book by Dr. B. R. Misra, Professor of Economics at the Benares Hindu University. It is in fact a thesis for the Ph.D. of London University and the book should be judged mainly from this point of view. In thirteen chapters a good deal of rough country is covered. After an introductory chapter on Indian economic conditions, the conditions of financial reform and local finance, follows an authoritative history of provincial finance with special reference to the developments that have taken place over the last two decades. Professor Misra then deals with the tax structure of the provinces and local authorities and in a final chapter summarises recent tendencies in provincial finance and makes some shrewd suggestions as to the future. There are two appendices, one on "Some Principles of Taxation" and the other on Legislative lists showing what the Centre and Provinces do and what taxes they levy. Mrs. Anstey, to whom many Indians owe a debt of gratitude, writes a model Foreword to the book. "Without her guidance," says the author, "the work would never have seen the light of day. It is impossible for me to express adequately my deep sense of gratitude to her."

The book gains by the special reference given to the United Provinces. In the past, in my judgment, there has always been the temptation to paint on too large a canvas so far as India is concerned. The theses submitted for the Ph.D. degree in London University by Indian students have frequently covered a very wide field. It is quite true that a man of genius may be able to see new conclusions in such wide fields, but the average investigator is surer of contributing to knowledge if he has a rigidly limited field of investigation. New and fresh materials carefully weighed will yield a rich harvest and add to our understanding of the development of the country as a whole. There must be not only new facts but new kinds of facts, or otherwise we shall get just another treatise of the well-known standardized sort giving little or nothing to, let us say, the science of finance or to the complicated economic world in which we live. The investigator is able to raise new questions and to explore new paths. He may have, and often does have, to retrace his footsteps, but he is usually openminded enough to allow new evidence to lead him in a new direction. I emphasise this aspect of research into Indian problems because the largest contributions to economics in India, as in other countries, must be always the product of individual scholarship and research. If research means, as I believe it does, the wresting from nature of her hidden uniformities by patient and methodical enquiry, it will be most successful where an industrious investigator, with the gift of imagination and fruitful ideas, undertakes work involving the gathering and careful scrutiny of new materials for a limited area on a rigidly defined subject. Dr. Misra was right in selecting for his study an Indian province, but perhaps in some future book he might give us a monographic work of the miniature type. I believe, for example, a study on local finance for a town or district could be a contribution of the first importance to our understanding of the development of a really little known part of Indian finance. When it comes to comparing district with district, province with province and city with city, there will be numerous issues, new facts and new kinds of facts, hitherto not discussed.

With Dr. Misra's criticisms of the first great step towards financial devolution under the Montagu-Chelmsford Reforms of 1919, most students of the subject will agree. The Meston Award did leave the Centre with the expanding sources of revenue, such as income tax, customs and salt, while the more inelastic heads of revenue, such as land revenue, went to the provinces. At the same time, as the author points out, the provincial revenues could have been expanded had even some of the recommendations of the Taxation Enquiry Committee been brought into operation. He is a strong advocate of the direct taxation of agricultural incomes and his remarks on prohibition deserve widespread attention. At the same time, a stronger case should perhaps have been made to show how both in central and provincial taxation the wealthier commercial classes are relieved of taxation to an unusual degree, and it was high time to redress this anomaly. At the Centre, too, in the pre-rearmament year 1937–38, 43 per cent. of the expenditure was on defence, or 22 per cent. of the total expenditure (central, provincial and local). In the United

States defence was 13 per cent., in Australia 8 per cent., in Canada 7 per cent. and in South Africa 5 per cent. of central expenditures. There are special reasons for high military expenditure in India, but when compared with education the expenditure is exceptionally high. The expenditures on education and the social services are relatively low. Great changes may be looked forward to in years to come in this direction. In 1937–38 education was 9 per cent. of the total expenditures of all branches of government as compared with, roughly, 16 per cent. in the United States, 20 per cent. in South Africa, 11 per cent. in Canada and 7 per cent. in Australia. The expenditures on the social services as a whole were in India 15 per cent. (education and public health), about 41 per cent. in the United States (including unemployment relief works), 36 per cent. in Canada, 29 per cent. both in South Africa and in Australia. Taxation, it may be noted, is largely on consumption and is regressive.

Chapter XII, on local taxation, is the least satisfactory in the whole book. It would have necessitated a careful and lengthy examination of the statistics of each province. In Dr. Misra's own words his aim was " to consider briefly the present situation and the lines along which the financial relations between the Provincial Governments and local authorities may be co-ordinated so that a greater element of efficiency, economy and harmony may be introduced into the tax system of the country ". The author makes several suggestions for improving local finance, but conditions differ so much from province to province in local finance that it is not easy to lay down uniform principles suitable to all local authorities, including the larger municipalities. All will agree with the view put forward that " The need for such a change was never so urgent as it is now ". When, however, we attempt to lay down a clearly cut system of functional reallocation coupled with the system of grants-in-aid our difficulties begin. It is a subject which the Centre and Provinces should take up with the least possible delay, and if Dr. Misra had his way local authorities would no longer have so free a hand in the matter of taxation. The grant-in-aid system would be, in his view, the best method of controlling their wayward policies.

The reviewer does not agree with the view held by the author in regard to the decay of village communities and to panchayats. The Report of the Royal Commission on Decentralization (1908) painted perhaps too rosy a picture of democratic institutions in ancient India. Recent historical research makes it clear that democracy in the sense that it is understood to-day did not exist until the advent of British rule and not in reality until comparatively recent times. Under the Mauryas, contemporary with Alexander the Great, there was an efficient but wholly autocratic State held together by militarism and espionage. A high degree of centralisation and officialdom was evident. Several centuries later under the Guptas no trace of democracy in the administration is to be found. In Vincent Smith's words, "The never-ending dynastic wars and revolutions did not bring about any development of political institutions. No republics were formed, no free towns were established. All the States continued to be governed in the old-fashioned way by despotic Rajas, each of whom could do what he pleased, so long as his power lasted, unless he suffered his will to be controlled by Brahman or other religious guides." Under the Mahomedan conquerors from the twelfth century for six centuries, subject like every generalisation about India to local reservations, no trace of democratic government is to be found. The old alliance between the warrior and Brahman priest continued and the one common feature was the despotic, often benevolent, rule everywhere but never dependent on the people's will. In regard to Panchayats or Village Elders Maine was most guarded in his description of their functions. He made no claim for them to have exercised executive or financial authority. He defined them as quasi-legislative and quasi-judicial. Later research has made it doubtful whether the village elders or council of five was a real administrative organ or just a tribunal for simple justice and the settlement of social disputes. The student of Indian finance cannot afford to overlook research in a sister science, and as Mill in his essay on Coleridge well said, we have still "the noisy conflict of half truths, angrily denying one another", and we must remember Mill's wise words: "All students of men and society who possess that first requisite for so difficult a study, a due sense of its difficulties, are aware that the besetting danger is not so much of embracing falsehood for truth, as of mistaking part of the truth for the whole."

G. FINDLAY SHIRRAS.

*World Economic Survey*, 1941–42. League of Nations, Geneva. 1942. 198 pp. 10s.

The League of Nations, in its tenth survey of world affairs, fully maintains the high level set in former years. It provides an indispensable collection, comparison and interpretation of the principal events, facts and figures affecting the economic position and development of the chief countries of the world during the third year of war.

Pride of place is naturally given to the economic concomitants of the "momentous developments" from the summer of 1941 to the autumn of 1942, including the German invasion of Russia, the Japanese conquests, and the transition to war economy in U.S.A. Some explanation of the remarkable Russian achievements is given by the fact that a comparatively high proportion of her productive resources were already engaged in the investment industries, which were well suited for conversion to war purposes, whilst the successful evacuation of productive capacity Eastwards was a continuation and acceleration of an already existing tendency. An admirable short sketch is given of the factors enabling the substantial industrial development which has occurred in interior China, where, for the first time in her history, she is building up a centralised fiscal and economic system. With regard to the Japanese position it is pointed out that although Japanese conquests have supplied her with surplus stocks of certain commodities—for instance, rubber, rice and tin—new problems (particularly the serious shipping shortage) and scarcities (for instance, of iron, steel and textiles) have arisen, whilst she is unable to offer the conquered territories a market large enough to compensate for the loss of their main former export markets.

One of the most interesting sections of the survey is devoted to an analysis of the Lend-lease policy. Mr. Roosevelt's statement of the principle upon which the financial burden of the war should be distributed (i.e. that if each country devotes roughly the same fraction of its national production to the war, then the real cost will be distributed fairly amongst the United Nations, in accordance with their ability to pay) might, surely, have called forth the comment that although this would be just as between countries with an approximately equal standard of life, it would involve a vastly greater real sacrifice from countries whose standard of life is much lower than that of the United States or Britain.

Special, though shorter, sections are devoted to developments in Britain, the Overseas Empire, and Latin America. A useful and concise analysis is given of Britain's national income and expenditure, from 1938 to 1941, in " real terms ", together with an estimate of the sources from which the increased Government expenditure (which is estimated to have risen from 17 per cent. to 52 per cent. of the national income between those years) has been derived. In dealing with the Overseas Empire interesting figures are given of the large-scale repatriation of debt and accumulation of sterling resources which are causing a revolution in the balance of indebtedness. In the future it is Britain that will be the debtor, whilst the Dominions and India will have become Britain's creditors.

The greater part of the information given by the Survey is grouped by subjects, rather than by geographical areas. Special chapters deal with production, consumption and rationing, finance and banking, price movements and control, international trade and transport. It is obviously impossible to comment separately here on each of these subjects. Let it suffice to say that the selection and presentation of facts and figures succeeds admirably in giving a vivid picture, placing events and developments in proper perspective and enabling valuable international comparisons. The detailed tables on such subjects as food-rations, government expenditure and revenue, note-circulation, commercial bank deposits, prices and trade are most useful for reference purposes.

One most interesting section describes German economic policy in the occupied countries, showing how Germany " pays " for its imports from these areas partly out of excessive occupation charges, partly by pure inflation. Germany is now taking full advantage of the position attained by exchange control and bilateral treaties, before the outbreak of war, to improve her terms of trade with Eastern European countries.

From the account of food production and rationing on the Continent it emerges that conditions there are progressively deteriorating and are far inferior, even in the most favoured districts, to those in this country. Switzerland was the last Continental country (except Portugal) to maintain an unrationed bread supply, but even there bread and milk rationing were introduced in October, 1942. In most countries even potatoes are rationed, whilst—owing to the unequal distribution of supplies—it is often impossible to procure the full rations. Hence—apart from the actual size of the rations—whereas in England more bread and potatoes can be obtained to compensate for the reduction in rationed foodstuffs (it is estimated that bread consumption has increased 20 per cent. in Britain), on the Continent there is no important foodstuff left unrationed to provide such compensation.

The Survey, covering as it does such a wide and varied field, is not, of course, free from defects. Two may be referred to here. The first is an almost complete lack of reference to the Jewish problem. No light is thrown on the rations obtainable by, or the means of livelihood open to Jews. The second is the nature of the population table on p. 19. This gives the actual births, deaths and natural increase since 1938 in seven countries. This method of presentation, in the absence of standardised birth- and death- rates, and without reference to pre-war trends, is at best unilluminating and at worst positively misleading. The figures as they stand provide no means of judging and comparing the effects of the war on population growth.

VERA ANSTEY.

## SHORTER NOTICES

*Introduction to Accounting for Students of Economics.* By C. A. ASHLEY. University of Toronto Press. 1942. 136 pp. $1.50.

This book may prove a useful basis for lectures or discussions on the borderline between economics and accounting, but is in itself rather too slight to be effective. The student of economics with no prior knowledge of accountancy will not derive from it, unassisted, any very clear picture of the inter-relationship of the books described, or of their use to build up the general analysis of a company's affairs which he is encouraged to attempt. On the other hand, its usefulness to him in his economic studies will be rather the negative one of providing warnings against the unconsidered use of financial statistics than the positive assistance towards the solution of theoretical problems, such as the relationship between capital and income, which such a book might afford.

*Wartime Rationing and Consumption.* League of Nations Economic Service. Allen & Unwin. 1942. 87 pp. 3s. 6d.

This pamphlet, expanded from a chapter in *World Economic Survey*, 1939–41, studies the effects of war on standards of living. Its four sections analyse all that was known in June, 1942, about rations and " points ", and the little that could be ascertained about their precise effect on actual consumption throughout the world.

To appreciate the figures requires the experience of a housewife rather than that of an economist, but of the conclusions suggested three at least point to factors of lasting significance. On the one hand, there is evidence that Governments have learnt to apply rationing systems both to satisfy more scientifically the needs of different classes of the community, and to bring nearer the acknowledged goal of a national minimum of subsistence for all. On the other, the low rations allotted to the occupied nations of Europe suggest a weakening, even more ruthless than that of 1914–18, in the stamina of an entire generation. Such points as these will need to be reckoned with in any effective plans for the post-war world.

The statistics given are adequately illustrated with diagrams ; two of these, however—on pp. 51, 52—differ from the accompanying text.

*Industrial Relations in Germany*, 1914–1939. By Waldo Chamberlin. Stanford University Press and Humphrey Milford. 1942. xv + 403 pp. 30s.

As far as it goes this is a useful book. The author has compiled an annotated bibliography of official documents, newspapers, journals and books dealing with industrial relations in Germany from 1914 to 1939, and covering some 2,000 different items. The brief synopses of the books are particularly helpful and the research worker in this field will have good reason to be grateful to Dr. Waldo Chamberlin. The principal defect of the book, and it is an important one, is that the material is limited to that which is available in the Hoover Library on War, Revolution, and Peace and in the Stanford University Library. Large though this material is, it is by no means comprehensive, and there are some remarkable and obvious lacunæ. However, if supplemented by the classified bibliographies given in Dr. Norman Baynes's recent edition of Hitler's speeches, the preliminary labour of discovering what has been written on this subject should be greatly lightened.

*The Fish Gate.* By Michael Graham. Faber and Faber, Ltd. 1943. 196 pp. 10s. 6d.

This survey of the English fishing industry by a marine biologist provides an admirable introduction to some fascinating problems strangely neglected by economists—considering that this is the main case where private property cannot be made the incentive to the preservation of natural resources. Though the emphasis is necessarily on the biological problems connected with the effects of over-fishing—which incidentally raise some interesting problems in the general theory of population—there are some excellent chapters on the more strictly economic aspects of fishing. Chapter III on "Markets" particularly is full of shrewd observations and good sense on the problem of distribution. And the description of the technical as well as the human aspects of the main branches of fishing is not only most instructive but a sheer delight to read.

*Explication Economique du Monde Moderne.* By WILHELM RÖPKE. Translation by PAUL BASTIER. Librairie de Medicis, Paris. 1940. 320 pp. 45 fr.

The first version of this book, *Die Lehre von der Wirtschaft*, published in Vienna in 1937, has already been reviewed in ECONOMICA.[1] The French translation, which bears the imprimatur of the French censorship for May 17, 1940, appears to have been actually published during 1942. The argument of the earlier version is somewhat elaborated, especially by the addition of an Introduction and a new final chapter. Professor Röpke condemns the claim of collectivists to be revolutionaries as something ridiculous, for they consistently refuse to go to the root of the matter, to be genuine radicals. To be a collectivist to-day, he says, is a pleasant way of escape for indolent reactionaries, and insisting that proletarianisation and monopoly are the real enemies, he pleads eloquently for a system of liberal interventionism, or interventionist liberalism, which will correct the grave errors into which economic policy has fallen in recent years, without running the still more serious risks which in his view are inevitably associated with the centralised control of economic life.

*Handbook of Latin American Studies: 1939.* Oxford University Press. 1940. 477 pp. 22s. 6d. net.

This is a selective bibliography of books, articles and official documents published in 1939 about Latin America, including the Caribbean area. The subjects covered are anthropology, archives, art, economics, education, folklore, geography, government, history, international relations, law, language and literature, libraries, music and philosophy. The Handbook is edited for the American Council of Learned Societies by Lewis Hanke and Miron Burgin, both of the Library of Congress. Similar Handbooks have been published for the four years preceding 1939; this is No. 5 in the Series. These Handbooks should be most useful to students of Latin American problems and progress.

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[1] Vol. IV (New Series), No. 16, November, 1937, pp. 470–2.

*The New Population Statistics.* By R. R. Kuczynski. National Institute of Economic and Social Research, Occasional Papers I. Cambridge: At the University Press. 1942. 31 pp. 1s. 6d.

In this pamphlet Dr. Kuczynski discusses the results presented by the Registrar-General in the Second Part of his 1938 Statistical Review, and in the White Paper on Current Population Trends in Great Britain. The former publication is unfortunately not easily accessible to the general public, and the reader would perhaps have liked to see a few more of the results, rather than a discussion of the various deficiencies of the statistics, most of which are not of very great importance. It is the second part of the pamphlet, dealing with the official population forecasts of the Registrar-General, that is by far the most valuable, and it admirably shows the meaning of the Registrar-General's assumptions and their deficiencies. The official optimism regarding our future population position is shown to be unfounded, and the pamphlet is a useful antidote which should be read by everyone who has had occasion to deal with the Registrar-General's figures.

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# Competition from Newcomers

By R. G. Hawtrey

DR. MACHLUP, in his two articles on " Competition, Pliopoly and Profit ", in the issues of ECONOMICA for February and May, 1943, has taken a step towards the formation of a theory of profit.

The limiting factor which prevents those established in an industry from exacting extortionate prices and profits is the potential competition of newcomers starting business in the industry. This potential competition Dr. Machlup calls pliopoly, the competition of " more sellers ", and distinguishes it from polypoly, the competition of " many sellers ". The market of many sellers is to be contrasted with that of a single seller, monopoly, and that of few sellers, oligopoly, few enough, that is, for each to have to reckon with the reactions of his operations on the others individually.

The term pliopoly is not very happily chosen as one of this set. Dr. Machlup indeed insists that a large number of sellers and an augmentable number of sellers belong to different spheres of thought. There can be a " polypolistic industry into which entry is not free "—And, he might have added, there can be monopolistic or oligopolistic industries into which entry *is* free. There may be only a single producer in an industry, and yet nothing to exclude rivals if they choose to start business in competition with him; the monopolist may be subject to the influence of pliopoly if he avoids exacting an extortionate profit, not because he fears that potential competitors can establish themselves, but because he prefers to avoid the trouble and expense of fighting them or buying them out.

The potential competition of newcomers is the primary competitive factor in the determination of profit. Competition from inside may be important. That is so when an industry is under-employed, and those engaged in it are struggling each to get as great a share as he can of an insufficient total of business. But in the normal case of a growing industry, demand is found from time to time to be outstripping capacity, and the immediate result is to enable all the producers to secure higher prices and augmented profit margins. It then becomes possible for a producer (whether from inside or from outside) to obtain a normal profit from additional productive capacity. If all the established producers prefer to hold back, perhaps from a tacit understanding that no one of them should endanger the excess profits that all are enjoying, then the initiative is likely to be taken from outside, and newcomers will bring the increased productive power.

That is what competition means in relation to enterprise and profit. And the crucial question is, what rate of profit will attract the competition of newcomers? That is the limiting rate, beyond which profit cannot be safely raised, and which therefore sets the standard of normal profit.

" The cost of a productive service," Dr. Machlup writes (p. 9), " is the value of its best alternative use, or, in other words, the ' opportunity ' which must be forgone when the resource is employed for a given purpose. Any number of potential applications ' compete ' for the use of the productive services. Losses arise when resources are allocated to fields in which the ' value product ' is below the values that could be produced by using the resources in other fields. These latter values, the sacrificed opportunities, are the costs of using the resources, or their productive services, in the given field. To find a field where the produced value is above the cost of the necessary productive services—that is to say, to find a field with economic net profits—means that a reallocation of resources, namely their entry into the ' profitable ' industry, is indicated. Perfect competition is supposed to bring this about. If it were done perfectly, there could be no net profit in the long run."

That is to say, profit, when it occurs, is either a symptom of disequilibrium, or a result of some restriction of competition. So long as competition is working effectively, the disequilibrium is temporary; it is corrected and the profit wiped out by the entry of newcomers into the industry. " Where pliopoly is absent because of excessive obstacles to entry, surpluses of receipts over long-run cost may endure. These ' remainders ' may have the character either of monopoly rent or of profit " (p. 10).

When all costs are said to be opportunity costs, the meaning is that the cost of using any factor is to be measured by the advantage that might have been derived from it in the most remunerative alternative use. (The most remunerative purpose for which it is *not* used is simply the marginal case.)

That is the principle which Dr. Machlup applies to all the productive resources assembled by the entrepreneur—except his own services. He regards any opening for enterprise which offers a return in excess of what these productive resources might yield in alternative uses as a temporary failure of equilibrium, and when the necessary movement of enterprise has occurred to restore equilibrium no enterprise will provide any residue for its promoter (except in the special case of obstructed entry of newcomers).

In this analysis he has neglected the opportunity cost of the entrepreneur's own services. One who chooses trade or business as a career has at the outset to compare its advantages with possible alternatives. He might qualify for a profession and derive an income from fees or a stipend, or he might obtain salaried employment. If he is wise, he will choose the occupation which suits his tastes and aptitudes, but he cannot neglect the pecuniary prospects. Surely if the normal remuneration of entrepreneurship (apart from monopolistic tricks which are not always practicable) is zero, it must be singularly unattractive ? Yet that is what Dr. Machlup would have us believe.

That is not an idiosyncrasy of his. It is what orthodox economists all over the world are teaching their students. There is no respect in which economic theory is in more urgent need of revision. Dr. Machlup's theory of pliopoly has the advantage of bringing the issue out into a clear light.

*Normal* profit in any trade or industry tends to be such as to offer a newcomer a reasonable income by comparison with the income he might get in other occupations. Newcomers cannot expect to start business on any but a moderate scale (except where they are introducing an innovation important enough to give a big advantage in selling and to attract large capital). The limiting factor is not so much capital resources as *selling power*. In an expanding trade the newcomer can get business without interfering with those already established, but he cannot expect to go further and get business away from them. Whatever devices in the way of price-cutting and intensified publicity he may resort to, they can do the same, and in the end, after having for a time had to forgo all or part of his profits, he will have gained nothing.

Selling power is in fact the foundation of profit-making. Given a sure prospect of selling the product, there is not likely to be any difficulty in raising capital.

Normal profit in any industry and in the whole range of trade and industry is that which will just offer newcomers a sufficiently attractive income on the moderate scale of selling power which is usually all they can count on.

That, it may be said, is a very indefinite standard. And so it is. Its indefiniteness is quite in accordance with the facts. It is not possible to estimate with any approach to exactitude just what percentage of turnover or of capital investment would be necessary to provide a reasonable middle-class income from the scale of business which a newcomer can expect. Nevertheless that *is* the standard which practical life imposes. It is a strange aberration of economic science to seek greater precision by substituting zero for the reasonable middle-class income.

When Dr. Machlup turns to the subject of uncertainty, he starts with the hypothesis that there are no other barriers against the flow of resources and none of the resources are indivisible. On that assumption he maintains that if full certainty prevailed about the future instead of uncertainty, and the gross receipts of every enterprise were expected with the fullest certainty, " profits could obviously not exist under these conditions. If the gross receipts from enterprise exceeded the sum of all prices of all the resources which are needed to produce the gross receipts, nothing would prevent more people from turning entrepreneur and buying or hiring (borrowing, leasing) more of the ' cheap ' resources. The demand for resources would stop rising only when the prices of the resources had increased enough to exhaust the full value product " (pp. 15–16). So attractive would be the prospect of earning nothing at all !

When the assumption of full certainty is abandoned, the entrepreneurs insist on " safety-margins ". " If gross receipts happen to come exactly up to expectations they will not only cover all costs of all resources, variable and fixed, hired and owned, but they will leave a remainder equal to the safety-margin: economic profit."

What is the meaning of " expectations " ? If there is a real risk of loss, and a safety-margin is reckoned on by way of an insurance premium against it, then so much loss as is equivalent to the safety-margin is *part of the expectation*. Against the profit secured by the happy venturer who incurs no such loss is to be set the loss which exceeds the safety-margin in the case of another. If the safety-margin is no more than a fair equivalent of the risk of loss, it is still true that an enterprise offers no prospective income at all (except bare interest on the capital engaged). The safety-margin is part of costs.

The assumption of " full certainty " includes certainty in regard to volume of sales and selling price. But that is not to be converted into the assumption that the product *will sell itself*, and that the anticipated volume of sales and selling price will be secured without interference or effort. Goods no more sell themselves than produce themselves. Certainty can only mean that the entrepreneur, given the skill and assiduity which he himself will put into the business, can count on the anticipated output and sales. And the income to be looked for will only be attractive if the income he will derive from the anticipated profit-margin on the sales will be sufficient. Otherwise he will not enter the business.

Obstructions in the way of competition from newcomers in an industry may fail to raise profits above normal. Normal profit-margin will still attract those newcomers who can surmount the obstacles. Only if the obstacles are so restrictive that insufficient new capacity is attracted into the industry to keep profits down to normal, or if the obstacles take the form of additional costs, will monopolistic advantage begin to appear.

Even when newcomers fail to appear, inside competition may still keep profits down to normal. A tacit accord will prevent established traders from trying to encroach on one another's sales so long as demand remains unchanged. But it is less likely to prevent them from competing for *new* demand. In fact, when demand expands, producers would probably be unable to prevent an extension of capacity and maintain excessive profits, except by express agreement. Even when a cartel or a monopoly is established, it may yet be found advantageous to forgo high profits on a restricted output, and cater for a large scale demand at moderate prices. A monopoly price may thus actually be less than a competitive price.

Dr. Machlup follows Professor F. H. Knight in distinguishing entrepreneurial and managerial functions. The managerial " can be expressed in physically measurable terms, such as the labour-time, the full working time of a manager; the entrepreneurial function is not reducible to any physically distinguishable units " (p. 14). Managerial ability can be contracted for in terms of man-hours or man-years, and derives its value from its productivity (p. 15). But entrepreneurial ability cannot be defined and measured by the profit which it supposedly explains " (p. 14).

Enterprise, in fact, does not conform to the principle of marginal yield, by which the remuneration of other factors of production is determined. An increase or decrease in the number of entrepreneurs does not necessarily mean an increase or decrease in production or in services rendered. A certain minimum number will be required to maintain the organisation of business; a larger number will be needed to organise business on a competitive basis. But once the minimum numbers needed are ensured, additional numbers may contribute little or nothing or even less than nothing to productive power. Thus there is no marginal entrepreneur corresponding to the workman whom it is just worth while to employ. There may be incompetent or unfortunate entrepreneurs who drop out, but the marginal workman's services are not excluded on the grounds of incompetence. They are excluded because his output would exceed demand at the remunerative price. The incompetent workman who is not worth his wage falls below the margin.

When an industry is over-extended, that means that too great productive capacity has been engaged in it, and that is possible whether the number of entrepreneurs be great or small. If, with a given volume of sales, the number of entrepreneurs becomes excessive, some of them may fail to gain a reasonable income; but that will happen in any case to some who are unlucky or deficient in selling power, even when the numbers are far from being excessive, and some of the survivors have many times the volume of business that would yield them good incomes.

The principle of marginal yield does apply at the point of contact between the career of profit-making entrepreneur and alternative occupations, but it is the marginal productivity of these latter that is involved. There is a close analogy between professional fees and business profits. The scale of fees in a profession must be such that the prospective professional income can be balanced against the income offered by salaried employment of similar grade. The successful barrister may get a very large income, but his position differs from that of the successful trader in that his product is a service in which his individual talents are an essential factor. The volume of business he can take (though it may be much greater than that done by the majority) is limited by the work he is personally capable of doing, and his high earnings are obtained from high fees. The individual quality of the service rendered is indeed a general characteristic of the professions. It is not entirely absent from trade. In so far as a trader's selling power is a positive contribution to economic welfare, that is because he has the discrimination, technical capacity and honesty to offer things of good quality or of desired quality for sale.

Thus high professional earnings are mainly and big business incomes partly rent of ability. In a sense indeed big business incomes might be classified as wholly rent of ability (except for the very considerable element of luck). For the qualities, good and bad, which go to make selling power are all " ability " in the economic sense. But to call the big profit-maker's income a rent of ability does not explain it away. In any form of salaried or wage-paid employment a rent of ability appears when the numbers available are insufficient to meet the demands of employers at ordinary rates of remuneration. A higher rate of pay excludes the demand of those employers who cannot afford to pay it, and corresponds to the demand of a marginal employer, who finds it just worth while to give it. The ability itself is likely to be graded, and the payment for services rendered will be the marginal value to those employing that grade.

Fee-paid services conform to the same principle. But the profit-making trader is on a different footing. He can expand his business and therefore his income almost indefinitely without finding his hands too full and his working day too short. It is usually possible to raise whatever capital is necessary to exploit the selling power he can show to be within his reach. Those who supply share capital get part of the profits, but the original promoters of a business are in a position to retain a disproportionate part for themselves. It is the mechanism of profit-making, making income proportional to turnover, which offers such an excessive rent of a kind of ability which in some aspects may be not at all desirable. The rent of ability is the excess over a normal income, and the normal income is the opportunity cost of the profit-maker who makes the choice between business enterprise and other vocations as a source of income.

These alternative vocations are assumed to be of the middle-class type, because the intending profit-maker must have some capital resources of his own. For some forms of small-scale profit-making (chiefly to be found in agriculture and retail business) the rival occupations are quite humble. But that does not mean that the profit-margin per unit of output or of sales is any the less. It may even be actually greater. For the more considerable enterprises the newcomer must bring an adequate capital, and, whether it is his own or he is trusted with it by others, he is marked as belonging to the propertied class, and will look for an appropriate income.

Profit is the remuneration of enterprise. Enterprise means any undertaking for the purpose of meeting demand. It is the meeting of demand that gives output the character of production. Selling power is therefore essential to enterprise. Profit is the margin between proceeds of sale and costs. It depends both on the price realised and on the magnitude of costs. Skill and care in reducing costs gain a reward in an increased margin of profit, which is a rent of ability. But this rent of ability, like the rest of profit, is proportional to *sales*. Selling power is the co-efficient on which the profit-maker's income above all depends.

# The Distribution of Incomes in the United States

By E. C. Rhodes

## I

The Pareto law of distribution of incomes is known to be fairly satisfactory for high incomes. It has been tested with good results on a variety of data. It is less satisfactory as a description of the distribution of incomes in the lower ranges. One suspects that no single simple law of distribution of incomes will serve over the whole range. In practice, the difficulty of trying different possible theoretical distributions is mainly due to the dearth of adequate data giving the complete distribution of incomes over the whole income range. There are available now, however, certain data relating to the United States. These data, which will be the subject of the present paper, relate to single incomes.[1] The irregularities in these figures, due to the method of estimating the data, have been smoothed out to a certain extent and the original figures can be reasonably well reproduced by certain mathematical formulæ. The notion of trying to find a single formula which will serve for the whole range of incomes has been abandoned, and it has been assumed that different laws operate for different income ranges.

The original data are shown in Table III. There are, in all, 10,058,000 incomes aggregating $11,579,390,000. We have split this whole group up into three parts:

(*a*) The Beak, consisting of 3,311,000 incomes aggregating $1,431,000,000, ranging from the lowest income (unspecified in the original table) to $1,000;

(*b*) The Head, consisting of 5,182,000 incomes aggregating $5,978,000,000, ranging from $259 to $7,500;

(*c*) The Tail, consisting of 1,565,000 incomes aggregating $4,171,000,000, ranging from $875 to the upper limit (unspecified in the original table).

The distribution of these three groups is shown in the table below (Table I).

Table I.

| Income Range ($) | Beak | | Head | | Tail | | All | |
|---|---|---|---|---|---|---|---|---|
| | Number | Amount ($000) | Number | Amount ($000) | Number | Amount ($000) | Number | Amount ($000) |
| —250 | 960,000 | 159,000 | | | | | 960,000 | 159,000 |
| 250—500 | 1,059,000 | 410,000 | 513,300 | 190,700 | | | 1,572,000 | 600,800 |
| 500—750 | 967,000 | 601,000 | 1,006,000 | 631,000 | | | 1,972,700 | 1,231,790 |
| 750—1,000 | 325,000 | 261,000 | 986,600 | 860,600 | 260,500 | 242,900 | 1,572,400 | 1,364,900 |
| 1,000—1,250 | | | 814,000 | 911,700 | 344,700 | 385,800 | 1,158,700 | 1,297,500 |
| 1,250—1,500 | | | 613,800 | 839,900 | 213,400 | 289,900 | 827,200 | 1,129,800 |
| 1,500—1,750 | | | 436,700 | 706,600 | 144,300 | 234,300 | 581,000 | 940,900 |
| 1,750—2,000 | | | 297,400 | 554,900 | 102,500 | 190,700 | 399,900 | 745,600 |
| 2,000—2,250 | | | 196,000 | 414,600 | 76,400 | 162,500 | 272,400 | 577,100 |
| 2,250—2,500 | | | 125,000 | 295,200 | 58,700 | 139,700 | 183,700 | 434,900 |
| 2,500—3,000 | | | 125,800 | 341,400 | 83,100 | 226,900 | 208,900 | 568,300 |
| 3,000—3,500 | | | 45,300 | 145,300 | 55,700 | 180,000 | 101,000 | 325,300 |
| 3,500—4,000 | | | 15,200 | 56,400 | 39,500 | 147,400 | 54,700 | 203,800 |
| 4,000—4,500 | | | 4,800 | 20,300 | 29,100 | 122,700 | 33,900 | 143,000 |
| 4,500—5,000 | | | 1,400 | 6,800 | 22,400 | 106,300 | 23,800 | 113,100 |
| 5,000—7,500 | | | 600 | 3,100 | 59,900 | 360,400 | 60,500 | 363,500 |
| 7,500—10,000 | | | | | 26,700 | 230,300 | 26,700 | 230,300 |
| 10,000—15,000 | | | | | 22,200 | 267,600 | 22,200 | 267,600 |
| 15,000—20,000 | | | | | 9,300 | 160,400 | 9,300 | 160,400 |
| 20,000—25,000 | | | | | 4,900 | 109,500 | 4,900 | 109,500 |
| 25,000—30,000 | | | | | 2,900 | 79,500 | 2,900 | 79,500 |
| 30,000—40,000 | | | | | 3,200 | 110,100 | 3,200 | 110,100 |
| 40,000—50,000 | | | | | 1,600 | 73,500 | 1,600 | 73,500 |
| 50,000— | | | | | 3,500 | 350,200 | 3,500 | 350,200 |
| Totals | 3,311,000 | 1,431,000 | 5,182,000 | 5,978,000 | 1,565,000 | 4,171,000 | 10,058,000 | 11,579,000 |

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[1] The Distribution of Single Incomes (1935–36) in the United States. *U.S. Statistical Abstract*, 1940.

The Beak consists of a large number of incomes, about one-third of the whole, within a very narrow range of income. The average incomes of those within the various income grades are:—

| Income Range ($) | Average Income ($) |
|---|---|
| — 250 | 165 |
| 250— 500 | 387 |
| 500— 750 | 621 |
| 750—1,000 | 804 |
| —1,000 | 432 |

We may reasonably infer that the 960,000 incomes below $250 range from about $80 to $250 and that the 325,000 incomes between $750 and $1,000 range up to about $860. Thus, this compact group of 3,311,000 incomes may be regarded as distributed fairly evenly in the range from $80 to $860. The argument in favour of this statement rests partly on the average incomes in the various grades and partly on the fact that there are about one million incomes in each of the grades under $250, $250–$500, $500–$750. It was not thought worth while to attempt to find a mathematical formula, which would describe the distribution of the Beak, partly on account of the small number of grades involved and partly because the data suggest that the distribution is fairly even throughout the range.

We note that the average income of the whole of the 3,311,000 in the Beak is $432.

The Head consists of just over half the total number of incomes and ranges from $259 to infinity. The average income of the Head is $1,154. For practical purposes the upper limit of the range is $7,500. Beyond this value the number of cases is infinitesimal. The equation of the frequency curve for the Head is

$$y = K a \beta e^{-\beta x^a} x^{a-2} \left(a \beta x^a - a + 1\right),$$

where $K = 5{,}834{,}500{,}000$, $a = 1.2891$, $\beta = .0001738$

This curve is zero when $x^a = \frac{a-1}{a\beta}$, i.e., when $x = 259$. It has a single mode when $x = 712$, i.e., the maximum frequency occurs for $712.

The Tail consists of about 15 per cent. of the total number of incomes and ranges from $875 to $2,280,000. The average income of the Tail is $2,666. The equation of the frequency curve for the Tail is

$$y = C\,(a - b \log x)^{\frac{1}{b} - 2}\, x^{-2}\,(a + 1 - b - b \log x),$$

where $C = 178{,}600{,}000$, $a = 3.8087$, $b = 0.260$

The maximum ordinate is that for $x = 875$.

Let us consider the degree of agreement between the theoretical distribution made up of the three specified parts and the original data. For convenience, the figures to be compared are shown together in Table II below.

For the lower income grades there is agreement between the two sets of figures, because no attempt has been made to find a theoretical curve to fit the Beak. For the other income ranges there are some large divergences between the smooth figures and the original data. Some of these differences exceed 20 per cent. of the theoretical figures.

Perhaps the more interesting features of the differences are those for the range $30,000—$40,000, where theoretically we expect about 3,200 incomes amounting to $110,100,000 and find about 2,400 incomes amounting to $80,882,000. There is possibly some compensation on this account in the surpluses in the preceding ranges of income from $20,000 to $25,000 and $25,000 to $30,000, where more incomes are shown in the original data than might be expected from our theoretical considerations. Similarly, there are fewer incomes recorded in the range $2,500 to $3,000 than might be expected and surpluses in the ranges $2,000 to $2,500 and $2,500 to $3,000. Again there are fewer incomes in the range $1,500 to $1,750 with an excess in the range $1,250 to $1,500, and fewer in the range $1,000 to $1,250 and an excess in the range $750 to $1,000.

Table II.

Original Data and Theoretical Distribution

| Income range ($) | Theoretical | | Original data | | Difference | | Difference % | |
|---|---|---|---|---|---|---|---|---|
| | Number | Amount ($000) | Number | Amount ($000) | Number | Amount ($000) | Number | Amount |
| Under 250 | 960,600 | 158,300 | 960,644 | 158,302 | + 0 | + 0 | — | — |
| 250—500 | 1,572,000 | 600,800 | 1,571,983 | 600,854 | − 0 | + 0 | — | — |
| 500—750 | 1,972,700 | 1,231,700 | 1,972,745 | 1,231,636 | + 0 | − 0 | — | — |
| 750—1,000 | 1,572,400 | 1,364,900 | 1,599,030 | 1,391,492 | + 26,600 | + 26,600 | + 1·7 | + 1·9 |
| 1,000—1,250 | 1,158,700 | 1,297,500 | 1,108,551 | 1,240,682 | − 50,100 | − 56,800 | − 4·3 | − 4·4 |
| 1,250—1,500 | 827,200 | 1,129,800 | 877,956 | 1,201,347 | + 50,800 | + 71,500 | + 6·1 | + 6·3 |
| 1,500—1,750 | 581,000 | 940,900 | 546,546 | 883,223 | − 34,500 | − 57,700 | − 5·9 | − 6·1 |
| 1,750—2,000 | 399,900 | 745,600 | 398,985 | 745,400 | − 900 | − 200 | − 0·2 | − 0·3 |
| 2,000—2,250 | 272,400 | 577,100 | 283,652 | 600,779 | + 11,300 | + 23,700 | + 4·2 | + 4·1 |
| 2,250—2,500 | 183,700 | 434,900 | 210,099 | 497,260 | + 26,400 | + 62,400 | + 14·4 | + 14·3 |
| 2,500—3,000 | 208,900 | 568,300 | 161,275 | 436,150 | − 47,600 | − 132,100 | − 22·8 | − 23·3 |
| 3,000—3,500 | 101,000 | 325,300 | 108,360 | 349,494 | + 7,400 | + 24,200 | + 7·3 | + 7·5 |
| 3,500—4,000 | 54,700 | 203,800 | 63,731 | 237,497 | + 9,000 | + 33,700 | + 16·4 | + 16·5 |
| 4,000—4,500 | 33,900 | 143,000 | 36,105 | 154,458 | + 2,200 | + 11,500 | + 6·5 | + 8·0 |
| 4,500—5,000 | 23,800 | 113,100 | 25,491 | 122,319 | + 1,700 | + 9,200 | + 7·1 | + 8·1 |
| 5,000—7,500 | 60,500 | 363,500 | 57,316 | 344,315 | − 3,200 | − 19,200 | − 5·3 | − 5·3 |
| 7,500—10,000 | 26,700 | 230,300 | 28,582 | 242,188 | + 1,900 | + 11,900 | + 7·1 | + 5·2 |
| 10,000—15,000 | 22,200 | 267,600 | 20,861 | 250,325 | − 1,300 | − 17,300 | − 5·9 | − 6.5 |
| 15,000—20,000 | 9,300 | 160,400 | 9,436 | 160,910 | + 100 | + 500 | + 1·1 | + 0·3 |
| 20,000—25,000 | 4,900 | 109,500 | 5,617 | 126,874 | + 700 | + 17,400 | + 14·3 | + 15·9 |
| 25,000—30,000 | 2,900 | 79,500 | 3,350 | 92,701 | + 500 | + 13,200 | + 17·2 | + 16·6 |
| 30,000—40,000 | 3,200 | 110,100 | 2,398 | 80,882 | − 800 | − 29,200 | − 25·0 | − 26·5 |
| 40,000—50,000 | 1,600 | 73,500 | 1,737 | 75,622 | + 100 | + 2,100 | + 6·2 | + 2·9 |
| 50,000 and over | 3,500 | 350.200 | 3,550 | 354,680 | + 100 | + 4,500 | + 2·9 | + 1·3 |

## II

## Mathematical Notes

Let us assume that the frequency distribution of incomes is $y = f(x)$, where $x$ is income, $f$ is an unknown function whose identity we try to determine from statistical data, and $y\,dx$ represents the number of persons with incomes between $x$ and $x + dx$. If $n$ represents the number of incomes above $x$,

$$n = \int_x^X f(x)\,dx,$$

and if $z$ represents the aggregate of all incomes above $x$,

$$z = \int_x^X x f(x)\,dx$$

In the above we have called the maximum possible income $X$, without making any effort to determine its exact value.

We note that $\frac{dz}{dx} = -x\,f(x)$, and $\frac{dn}{dx} = -f(x)$. Also, from the definition of $z$, we have

$$z = -\int_x^X x\frac{dn}{dx}\,dx = nx + \int_x^X n\,dx$$

We will call $\int_x^X n\,dx = w$. This function $w$ will be useful later. We note that $\frac{dw}{dx} = -n$ and $w = z - nx$. Also we note that $\frac{nx}{w} = -\frac{x}{w}\frac{dw}{dx} = -\frac{d(\log w)}{d(\log x)}$

From our data, relating to incomes, we have tabulated values of $n$ and $z$ for various values of $x$ and we can compute $\frac{w}{nx} = \frac{z}{nx} - 1$ for the various values of $x$. Thus, we can observe, by plotting the values of $\frac{w}{nx}$ against the appropriate values of $x$, what kind of functional relationship appears to exist between $\frac{w}{nx}$ and $x$. Hence, we can get an equation $\dfrac{d(\log w)}{d(\log x)}=$ some function of $x$, and from this by integrating, we can determine $w$ as a function of $x$. Thence we can find $n$ and $z$ and $y$ in terms of $x$.

As a preliminary, it is useful to consider the Pareto distribution. For this, we may write

$$n = Ax^{-p}$$

$$f = -\frac{dn}{dx} = Apx^{-p-1}, \text{ and } z = \frac{Ap}{p-1}\,x^{-p+1}$$

$$\text{Hence } \frac{z}{nx} = \frac{p}{p-1}, \text{ therefore } \frac{w}{nx} = \frac{1}{p-1}$$

Thus the plot of values of $\frac{w}{nx}$ against $x$ will give us a horizontal straight line. Any divergence from the Pareto law will easily be visible in such a diagram.

The present technique is similar to that of the Lorenz graph plotted on a logarithmic scale. In the Lorenz graph $z$ is plotted against $n$. If the Pareto Law held, the resulting graph would be a straight line given by

$$p \log z = (p-1) \log n + \text{constant}.$$

Where the Pareto Law does not hold, we sometimes find that a Lorenz graph (plotting $\log z$ against $\log n$) indicates a curve which does not differ much from a straight line. It is difficult to infer from such a graph what is the more exact connection between $z$ and $n$. There is also the disadvantage that $x$ has been eliminated temporarily and one has no particular desire to express $\log z$ as a function of $\log n$. By combining $z$ and $n$ in the form $\frac{w}{nx}$ and expressing the result as a function of $x$, we are able to proceed more easily to the discovery of the laws connecting $n$ and $z$ with $x$.

TABLE III.

U.S. DISTRIBUTION OF SINGLE INCOMES 1935–36

| (1) ($x$) Income ($) | (2) ($n$) Number of Incomes | (3) ($z$) Aggregate Income ($000) | (4) $w/nx$ | (5) Values of $n$ formula A | (6) Values of $z$ formula B ($000) | (7) (2)–(5) Remainder ($n$) | (8) (3)–(6) Remainder ($z$) ($000) |
|---|---|---|---|---|---|---|---|
| Unknown lowest income | Total number 10,058,000 | Grand total 11,579,390 | | | | | |
| 250 | 9,097,356 | 11,421,088 | 4·016 | 8,334,000 | 7,027,000 | 763,000 | 4,394,000 |
| 500 | 7,525,373 | 10,820,234 | 1·876 | 3,328,000 | 5,310,000 | 4,198,000 | 5,510,000 |
| 750 | 5,552,628 | 9,588,598 | 1·302 | 1,929,000 | 4,464,000 | 3,624,000 | 5,124,000 |
| 1,000 | 3,953,598 | 8,197,106 | 1·073 | 1,304,000 | 3,928,000 | 2,650,000 | 4,269,000 |
| 1,250 | 2,845,047 | 6,956,424 | 0·956 | 959,400 | 3,542,000 | 1,886,000 | 3,415,000 |
| 1,500 | 1,967,091 | 5,755,077 | 0·950 | 745,900 | 3,252,000 | 1,221,000 | 2,503,000 |
| 1,750 | 1,420,545 | 4,871,854 | 0·960 | 601,700 | 3,018,000 | 818,900 | 1,854,000 |
| 2,000 | 1,021,560 | 4,126,454 | 1·019 | 499,200 | 2,827,000 | 522,400 | 1,299,000 |
| 2,250 | 737,908 | 3,525,675 | 1·124 | 422,800 | 2,664,000 | 315,100 | 861,000 |
| 2,500 | 527,809 | 3,028,415 | 1·297 | 364,100 | 2,525,000 | 163,700 | 504,000 |
| 3,000 | 366,534 | 2,592,265 | 1·357 | 281,000 | 2,298,000 | 85,500 | 294,000 |
| 3,500 | 258,174 | 2,242,771 | 1·481 | 225,300 | 2,118,000 | 32,900 | 125,000 |
| 4,000 | 194,443 | 2,005,274 | 1·577 | 185,800 | 1,970,000 | 8,600 | 35,000 |
| 4,500 | 158,338 | 1,850,816 | 1·597 | 156,700 | 1,848,000 | 1,700 | 3,000 |
| 5,000 | 132,847 | 1,728,497 | 1·602 | 134,300 | 1,741,000 | | |
| 7,500 | 75,531 | 1,384,182 | 1·443 | 74,430 | 1,381,000 | | |
| 10,000 | 46,949 | 1,141,994 | 1·433 | 47,700 | 1,151,000 | | |
| 15,000 | 26,088 | 891,669 | 1·279 | 25,520 | 883,100 | | |
| 20,000 | 16,652 | 730,759 | 1·194 | 16,190 | 722,800 | | |
| 25,000 | 11,035 | 603,885 | 1·189 | 11,280 | 613,200 | | |
| 30,000 | 7,685 | 511,184 | 1·218 | 8,365 | 533,800 | | |
| 40,000 | 5,287 | 430,302 | 1·035 | 5,161 | 423,700 | | |
| 50,000 | 3,550 | 354,680 | 0·998 | 3,513 | 350,200 | | |
| 100,000 | 1,080 | 201,212 | 0·863 | | | | |
| 250,000 | 272 | 102,760 | 0·511 | | | | |
| 500,000 | 55 | 38,436 | 0·398 | | | | |
| 1,000,000 | 12 | 14,587 | 0·216 | | | | |

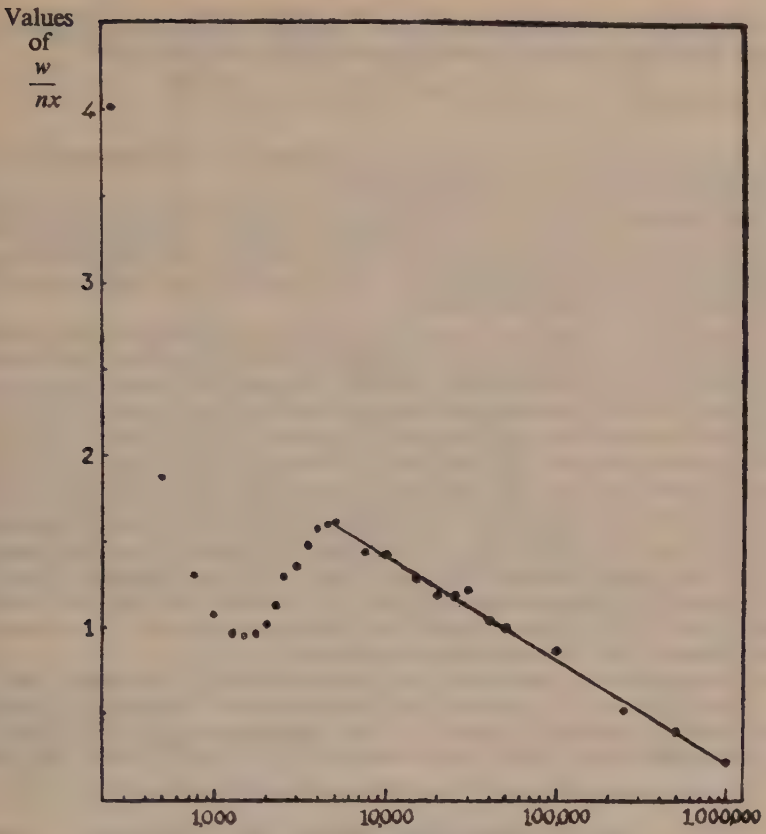

Fig. 1

Table III shows our data, giving $n$ and $z$ for successive values of $x$ together with the derived values of $\frac{w}{nx}$. These values of $\frac{w}{nx}$ are plotted against $x$ in Fig. 1, $x$ being on a logarithmic scale. There is no doubt from this diagram that the Pareto Law is not appropriate. It is difficult to determine from the diagram what the connection between $\frac{w}{nx}$ and $\log x$ is. There appear to be two turning points in $\frac{w}{nx}$ which suggests the possibility of a cubic. On the other hand, that part of the relationship between $\frac{w}{nx}$ and $\log x$ for values of $x$ from about \$5,000 appears to be approximately linear. The procedure followed was therefore to fit a linear relation between $\frac{w}{nx}$ and $\log x$ for values of $x$ from \$5,000 upwards. We found $\frac{w}{nx} = 3.8087 - 0.599 \log_{10} x$, for $x$ ranging from \$5,000 to \$1,000,000. We can write $-\frac{d(\log w)}{d(\log x)} = \frac{1}{3.8087 - 0.260 \log_e x}$

From which we get $w = C(a - b\log x)^{\frac{1}{b}}$

$$\text{Hence } n = \frac{C}{x}(a - b\log x)^{\frac{1}{b}-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(A)}$$

$$z = C\left\{(a - b\log x)^{\frac{1}{b}} + (a - b\log x)^{\frac{1}{b}-1}\right\} \quad . \quad . \quad . \quad . \qquad \text{(B)}$$

$$\text{and} \qquad y = \frac{C}{x^2}(a - b\log x)^{\frac{1}{b}-2}(1 + a - b - b\log x).$$

Where $C = 178{,}600{,}000$, $a = 3.8087$, $b = 0.260$. We may note that if $b$ is small compared with $a$, so that $b \log x$ is small compared with $a$, we may write

$$\left(a - b \log x\right)^{\frac{1}{b}-1} = \left(a\, e^{-\frac{b}{a}\log x}\right)^{\frac{1}{b}-1} = a^{\frac{1}{b}-1} x^{-\frac{1}{a}+\frac{b}{a}}$$

Thus, approximately, $n = K\, x^{-\frac{1}{a}-1}$, where $K$ is a constant, i.e., our expression for $n$ reduces to the Pareto form when $b$ is sufficiently small.

We find the formulæ ($A$, $B$) for $n$ and $z$ deduced above satisfactory when compared with the original data for values of $x$ from \$5,000 to \$50,000; for the extreme tail of the distribution from \$100,000 upwards the fit is not so good. In what follows we have not bothered to deal with the extreme tail. The number of cases above \$100,000 is just over 1,000 out of a total of 10 millions.

In Table III we have inserted the values of $n$ and $z$ computed from our formulæ. It will be seen that there is quite a close correspondence between the theoretical values and the original values for $x$ from \$5,000 to \$50,000. We will call this distribution the Tail. The values of $n$ and $z$ are also shown in this table for lower values of $x$. If we assume that these formulæ for $n$ and $z$ account for some of the incomes of our data below \$5,000 we can obtain the remainder by subtracting the computed values of $n$ and $z$ from the original values. This has been done in Table III. Columns (7) and (8) of that Table give us the remainder of the distribution after allowing for that given by formulæ $A$ and $B$. It is apparent that these formulæ cannot be allowed to represent the facts, even in part, when $x$ is \$250. The figures in columns (7) and (8) would require negative frequencies between \$250 and \$500. Thus, our formulæ $A$ and $B$ must not be supposed to operate below $x =$ \$500. It is obvious that the distribution given by our formulæ $A$ and $B$ must start at some minimum value of $x$. We can get a hint of this minimum from the following considerations.

Let us call the theoretical values of $n$ and $z$ obtained from the formulæ $A$ and $B$ for a given value of $x$, $\nu$ and $\xi$. Let us call the total number of persons in the whole distribution $N$ and the aggregate income $Z$. Then supposing our tail distribution to start at $x$, the remainder, the head, would consist of $N - \nu$ persons with aggregate income $Z - \xi$. The average income, $\bar{x}$, of the head would be given by $\bar{x} = \frac{Z - \xi}{N - \nu}$, or $\bar{x}\,(N - \nu) = Z - \xi$. Variations would take place in $\bar{x}$ with variations in $x$. These would be determined by the equation $\frac{d\bar{x}}{dx}(N - \nu) - \bar{x}\frac{d\nu}{dx} = -\frac{d\xi}{dx}$. Since $\frac{d\xi}{dx} = -xy$ and $\frac{d\nu}{dx} = -y$, this equation may be written $\frac{d\bar{x}}{dx}(N - \nu) = (x - \bar{x})\,y$. From this we see that $\bar{x}$ increases with $x$ so long as $x > \bar{x}$ and will decrease with $x$ when $x < \bar{x}$. If we took the start of the tail of the distribution at such a low value of $x$ that the average of the head was greater than $x$ it would mean that the head would be like another tail to the distribution. It seems reasonable to take as a minimum value of $x$ that for which $\frac{d\bar{x}}{dx} = 0$. This occurs when $\bar{x} = x$ or when $Z - \xi = (N - \nu)x$, i.e., when $w = Z - Nx$, where $Z = 11{,}579{,}390{,}000$, $N = 10{,}058{,}000$. From our formula for $w$ we find the critical value of $x$ to be \$875. We will therefore assume that the formulæ $A$ and $B$ apply for values of $x$ above this, i.e., we will assume that the tail of the distribution starts at \$875. From our formulæ the values of $n$ and $z$ for $x = 875$ are $n = 1{,}564{,}600$, $z = 4{,}170{,}600{,}000$. The remainder or head of the distribution will therefore be given by the figures in Table IV. We also show $\frac{w}{nx}$ for these values.

An examination of the values of $w/_{nx}$ in Table IV shows that they are certainly not constant. In fact, when they are represented graphically on a double log-scale in Fig. 2, we see that there appears to be a linear connection between the logs. We get, concentrating on the range of $x$ from \$1,000 to \$3,500, $w/_{nx} = 10^{3{\cdot}6493}\; x^{-1{\cdot}2891}$

From this, remembering that $\frac{dw}{dx} = -n$, we get by integration,

$$w = 5{,}834{,}500{,}000\; e^{-{\cdot}0001738x^{1{\cdot}2891}}$$

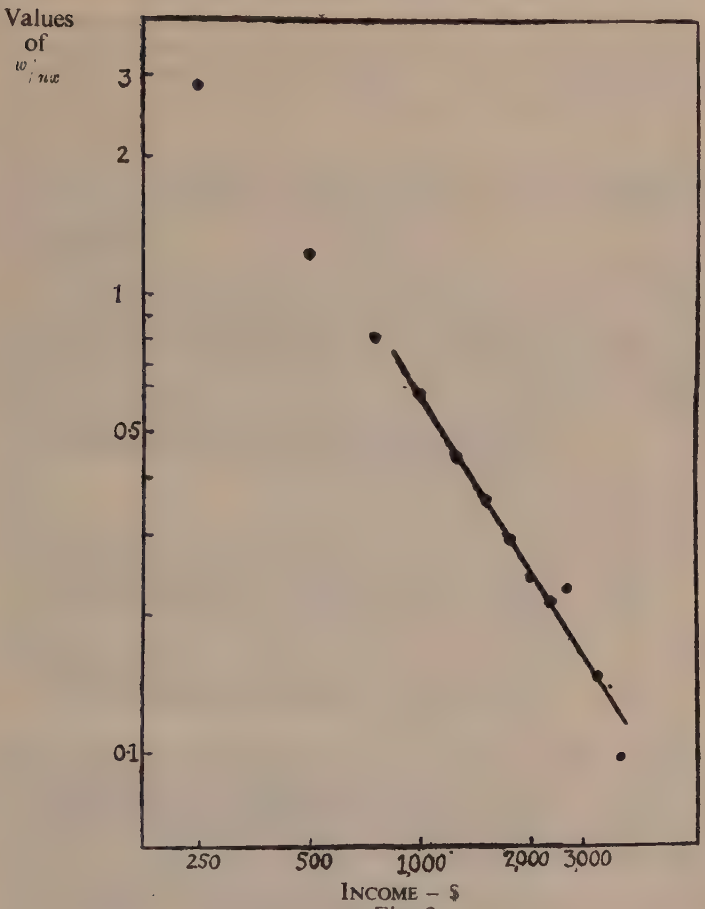

Fig. 2

Let us write $w = K e^{-\beta x^{a}}$

Then $n = K a \beta x^{a-1} e^{-\beta x^{a}}$ . . . . . . . . . . . . . . C

and $z = K e^{-\beta x^{a}} (1 + a \beta x^{a})$ . . . . . . . . . . . . . D

Further, $y = K a \beta e^{-\beta x^{a}} x^{a-2} \left(a \beta x^{a} - \overline{a-1}\right)$

Thus the frequency curve, $y$, is zero when $x^{a} = \frac{a-1}{a\beta}$, i.e., where $x = 259$.

TABLE IV.

| ($x$)<br>Income ($) | ($n$)<br>Number | ($z$)<br>Aggregate Income ($000) | $w/nx$ |
|---|---|---|---|
| — | 8,493,000 | 7,409,000 | — |
| 250 | 7,533,000 | 7,251,000 | 2·850 |
| 500 | 5,961,000 | 6,650,000 | 1·231 |
| 750 | 3,988,000 | 5,418,000 | 0·812 |
| 1,000 | 2,650,000 | 4,269,000 | 0·611 |
| 1,250 | 1,886,000 | 3,415,000 | 0·499 |
| 1,500 | 1,221,000 | 2,503,000 | 0·361 |
| 1,750 | 818,900 | 1,854,000 | 0·294 |
| 2,000 | 522,400 | 1,299,000 | 0·244 |
| 2,250 | 315,100 | 861,000 | 0·215 |
| 2,500 | 163,700 | 504,000 | 0·231 |
| 3,000 | 85,500 | 294,000 | 0·148 |
| 3,500 | 32,900 | 125,000 | 0·086 |
| 4,000 | 8,600 | 35,000 | — |
| 4,500 | 1,700 | 3,000 | — |
| 5,000 | 0 | 0 | — |

TABLE V.

| | TAIL | | HEAD | | HEAD AND TAIL | | ORIGINAL | | REMAINDER (BEAK) AND DIFFERENCES | | DIFFERENCE PER CENT. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (x) Income | (n) Number | (z) Aggregate Income ($000) | (n) Number | (z) Aggregate Income ($000) | (n) Number | (z) Aggregate Income ($000) | (n) Number | (z) Aggregate Income ($000) | (n) Number | (z) Aggregate Income ($000) | (n) Number | (z) Aggregate |
| — | 1,565,000 | 4,171,000 | 5,182,000 | 5,978,000 | 6,747,000 | 10,149,000 | 10,058,000 | 11,579,000 | + 3,311,000 | + 1,431,000 | — | — |
| 250 | 1,565,000 | 4,171,000 | 5,182,000 | 5,978,000 | 6,747,000 | 10,149,000 | 9,097,000 | 11,421,000 | + 2,351,000 | + 1,272,000 | — | — |
| 500 | 1,565,000 | 4,171,000 | 4,669,000 | 5,788,000 | 6,233,000 | 9,958,000 | 7,525,000 | 10,820,000 | + 1,292,000 | + 862,000 | — | — |
| 750 | 1,565,000 | 4,171,000 | 3,663,000 | 5,157,000 | 5,227,000 | 9,327,000 | 5,553,000 | 9,589,000 | + 325,000 | + 261,000 | — | — |
| 1,000 | 1,304,000 | 3,928,000 | 2,676,000 | 4,296,000 | 3,980,000 | 8,224,000 | 3,954,000 | 8,197,000 | − 26,600 | − 26,600 | − 0·7 | − 0·3 |
| 1,250 | 959,400 | 3,542,000 | 1,862,000 | 3,384,000 | 2,821,000 | 6,926,000 | 2,845,000 | 6,956,000 | + 23,600 | + 30,200 | + 0·8 | + 0·4 |
| 1,500 | 745,900 | 3,252,000 | 1,248,000 | 2,544,000 | 1,994,000 | 5,796,000 | 1,967,000 | 5,755,000 | − 27,100 | − 41,300 | − 1·4 | − 0·7 |
| 1,750 | 601,700 | 3,018,000 | 811,500 | 1,838,000 | 1,413,000 | 4,856,000 | 1,421,000 | 4,872,000 | + 7,400 | + 16,300 | + 0·5 | + 0·3 |
| 2,000 | 499,200 | 2,827,000 | 514,200 | 1,283,000 | 1,013,000 | 4,110,000 | 1,022,000 | 4,126,000 | + 8,200 | + 16,600 | + 0·8 | + 0·4 |
| 2,250 | 422,800 | 2,664,000 | 318,200 | 868,400 | 741,000 | 3,533,000 | 737,900 | 3,526,000 | − 3,100 | − 7,100 | − 0·4 | − 0·2 |
| 2,500 | 364,100 | 2,525,000 | 193,200 | 573,200 | 557,300 | 3,098,000 | 527,800 | 3,028,000 | − 29,500 | − 69,400 | − 5·3 | − 2·2 |
| 3,000 | 281,000 | 2,298,000 | 67,360 | 231,800 | 348,300 | 2,529,000 | 366,500 | 2,592,000 | + 18,200 | + 62,800 | + 5·2 | + 2·5 |
| 3,500 | 225,300 | 2,118,000 | 22,060 | 86,490 | 247,300 | 2,204,000 | 258,200 | 2,243,000 | + 10,800 | + 38,500 | + 4·9 | + 1·8 |
| 4,000 | 185,800 | 1,970,000 | 6,841 | 30,140 | 192,700 | 2,000,000 | 194,400 | 2,005,000 | + 1,800 | + 4,800 | + 0·9 | + 0·2 |
| 4,500 | 156,700 | 1,848,000 | 2,017 | 9,866 | 158,700 | 1,858,000 | 158,300 | 1,851,000 | − 400 | − 6,700 | − 0·2 | − 0·4 |
| 5,000 | 134,300 | 1,741,000 | 568 | 3,054 | 134,900 | 1,744,000 | 132,800 | 1,728,000 | − 2,000 | − 15,900 | − 1·5 | − 0·9 |
| 7,500 | 74,400 | 1,381,000 | 0 | 0 | 74,430 | 1,381,000 | 75,530 | 1,384,000 | + 1,100 | + 3,200 | + 1·5 | + 0·2 |
| 10,000 | 47,700 | 1,151,000 | | | 47,700 | 1,151,000 | 46,950 | 1,142,000 | − 750 | − 8,700 | − 1·6 | − 0·8 |
| 15,000 | 25,520 | 883,100 | | | 25,520 | 883,100 | 26,090 | 891,700 | + 570 | + 8,500 | + 2·2 | + 1·0 |
| 20,000 | 16,190 | 722,800 | | | 16,190 | 722,800 | 16,650 | 730,800 | + 460 | + 8,000 | + 2·9 | + 1·1 |
| 25,000 | 11,280 | 613,200 | | | 11,280 | 613,200 | 11,040 | 603,900 | − 250 | − 9,400 | − 2·2 | − 1·5 |
| 30,000 | 8,365 | 533,800 | | | 8,365 | 533,800 | 7,685 | 511,200 | − 680 | − 22,600 | − 8·2 | − 4·3 |
| 40,000 | 5,161 | 423,700 | | | 5,161 | 423,700 | 5,287 | 430,300 | + 126 | + 6,600 | + 2·4 | + 1·6 |
| 50,000 | 3,513 | 350,200 | | | 3,513 | 350,200 | 3,550 | 354,700 | + 37 | + 4,500 | + 1·1 | + 1·3 |

This curve therefore starts at \$259.

The curve theoretically extends to infinity, but for practical purposes the ordinates are negligible for values of $x$ near \$7,500. The curve has a maximum ordinate when $x=$ \$712. The total number of incomes in this distribution is 5,182,000 and the aggregate income of all in this distribution is \$5,978,000,000. We will call this distribution the Head.

Let us now compute the appropriate values of $n$ and $z$ from formulæ *C* and *D* and tabulate them with the values of $n$ and $z$ for the Tail. These are shown in Table V. Together, the Head and Tail account for 6,747,000 incomes aggregating \$10,149,000,000.

In Table V we also show a comparison between the combined Head and Tail distributions and the original data. There are large differences for the initial values of $x$, up to \$750, thereafter the differences, which are also shown in percentage form in Table V, are of small dimensions except for particular values of $x$, e.g., \$2,500, \$3,000 and \$3,500 and \$30,000. The initial differences, we will assume, are due to the existence of the Beak. We shall not attempt any further analysis on this part of the distribution as the number of known values of $x$ is small. We shall therefore regard the Beak as consisting of the following distribution.

THE BEAK.

| Income (\$) | Number | Aggregate Income (\$000) | Average Income (\$) |
|---|---|---|---|
| Under 250 | 960,000 | 159,000 | 165 |
| 250—500 | 1,059,000 | 410,000 | 387 |
| 500—750 | 967,000 | 601,000 | 621 |
| 750—1,000 | 325,000 | 261,000 | 804 |
| TOTAL .. | 3,311,000 | 1,431,000 | 432 |

As was stated in Part I, the numbers in the various income grades and the average incomes suggest that the distribution is fairly even. We may infer that the lowest income is about \$80 and the highest about \$860.

We conclude then with a fairly satisfactory description of the original data by two frequency curves defining the Head and the Tail, together with frequencies, described as the Beak.

It is of interest to consider further the frequency curves which emerge from the preceding discussion as the curves describing the Head and the Tail.

We can see, at a glance, from Table I that both these curves are skew. An interesting comparison is afforded by the following table giving percentiles and similar measures for the two curves.

TABLE VI.

| Percent. of Total Frequency | Value of $x$ (\$) | | Ratio. Tail ÷ Head |
|---|---|---|---|
| | Head | Tail | |
| 80 | 634 | 1,030 | 1·63 |
| 70 | 758 | 1,136 | 1·50 |
| 60 | 886 | 1,270 | 1·43 |
| 50 | 1,023 | 1,449 | 1·42 |
| 40 | 1,179 | 1,701 | 1·44 |
| 30 | 1,365 | 2,088 | 1·53 |
| 20 | 1,610 | 2,784 | 1·73 |
| 10 | 1,995 | 4,504 | 2·26 |
| 5 | 2,354 | 7,204 | 3·06 |
| 1 | 3,119 | 20,420 | 6·55 |

For the Head the median is \$1,023, for the Tail it is \$1,449, these being in ratio 1 to 1.42. Twenty per cent. of the incomes in the Head distribution are below \$634, the corresponding income for the Tail is \$1,030, these incomes are in ratio 1 to 1.63. When we come to the more exceptional incomes, 10 per cent. of those in the Head are greater than \$1,995 and in the Tail, \$4,504. The ratio here is 2.26. The fortunate few, 1 per cent., of the Head get incomes greater than \$3,119. The corresponding fortunate group in the Tail have incomes more than \$20,420. The ratio is now 1 to 6.55. We observe how this ratio of comparable incomes of the two groups rises quickly from a general level of about 1½ for the lower incomes in each group up to about 6½ for the 1 per cent. with highest incomes in each group. These figures are indicative of the different laws which are operating in the distribution of incomes in the two groups designated as the Head and the Tail. The most gifted persons in the Head get incomes more than \$3,119 which is three times the median income of the group. The most gifted of those in the Tail get incomes more than \$20,420 which is fourteen times the median income of this group.

The difference between the Head and the Tail is like the difference between an arithmetic progression and a geometric progression. We could, theoretically, obtain a correspondence between the $x$'s for the Head and Tail curve from the equations of these curves, but as these equations involve somewhat complicated mathematical functions, the results would convey very little. But, we can show that there is an approximate connection between them of a nature like that used by Gibrat. That is, we find that if we use the equation $x_H = A \log (x_T - x_o) + B$, where $X_H$ and $X_T$ are the Head and Tail incomes respectively, and $A$, $B$, $x_o$ are constants, we can transform the Head curve into the Tail curve and vice versa, at any rate to some fair degree of approximation. This equation is illustrated quite well in Table VI. The equation shows that absolute changes in income in the Head group correspond to relative changes in income in the Tail group.

# Gibson and Johnson: a Forgotten *Cause Celèbre*

By J. K. Horsefield

It is a commonplace that in late eighteenth-century Lancashire regular bank-notes were little used, the monetary circulation comprising chiefly bills of exchange. "At Liverpool and Manchester," says Thornton, "all Payments are made either in Coin or in Bills of Exchange."[1] Such a practice had its obvious dangers, in days when the art of "drawing and re-drawing" bills was very well understood,[2] but concrete illustrations of its extent and effects are not very numerous. It may therefore be worth while to set out the circumstance of one such case, which has come to light through the pursuit of a chance reference in Sir Francis Baring's *Observations on the Establishment of the Bank of England . . .*" (1797).

It appears from an unpublished MS. "History of Printworks in the Manchester District from 1760 to 1846" by John Graham[3] that shortly before 1780 some premises at Mosney, near Preston, were taken over and largely extended by a firm which came to be known as Livesey, Hargreaves, Anstie, Smith and Hall.[4] They traded, under the name of the Mosney Company, as cloth-printers and "continued to extend until they employed all the country". From other sources we know they were cotton-spinners at Clithero and had a London house in Cheapside.[5] They "were large manufacturers, as well as printers" (to quote Graham again), "were bankers, issued their own notes, made them to suit various purposes. If a weaver brought in a piece that was 2s. 6d. for weaving he received a note of 2s. 6d. value and so on—if a farmer did them work he received notes to the amount of work done, their notes were received with great eagerness—they were the circulating medium of the country in and about Preston—men of all trades and callings had them in possession." But Livesey and Co. fell; and in their fall they brought down so many other houses that, says Graham, "their failure convulsed the whole country for some time."

One example, both of the causes and of the effects of their collapse, can be found in the case of *Collis and Others v. Emett* already quoted, which was argued in the Court of Common Pleas in Trinity and Michaelmas terms, 1789. The special verdict disclosed that John Emett, partner with Livesey and Co. at Clithero, wrote his name on a piece of blank paper, with a shilling stamp thereon, and gave it to Livesey and Co., for purposes of drawing a bill of exchange. On April 5th, 1788, a creditor of Livesey and Co., one Thomas Jeffrey, produced to them for payment a bill for £1,512 9s. 0d. which had matured. He was persuaded to take, instead of cash, a fresh bill, for £1,551 (including three months' interest at over 10 per cent. per annum), payable on July 5th. For the creation of this bill, Mr. Emett's piece of paper was used, the bill being drawn on Livesey and Co., and made payable to a Mr. George Chapman, who did not exist. The bill was accepted by Livesey and Co., endorsed by them in Chapman's name, and handed to one Collis, clerk to Thomas Jeffrey, by whom it was discounted with the plaintiffs. Three weeks before it matured, Livesey and Co. appeared as bankrupt in the *London Gazette*.[6] The discounter sued Emett, and obtained a verdict on February 12th, 1790.

It is clear from the circumstances of this case that by the spring of 1788 the great house of Livesey and Co. was heading for the rocks. Such expedients as that by which Jeffrey was bought off argue a desperate familiarity with the shifts of financial recklessness. But it was not so much their general use of accommodation paper as their invention of imaginary payees which brought their name before the Courts and led them, all unwitting, to occasion the enunciation of a legal principle of some importance in the law of bills of exchange.

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[1] *Evidence to Lords' Committee*, 1797 (Hayek's edition, p. 296).

[2] *Wealth of Nations*, Bk. II, Chap. II.

[3] P. 407. I am indebted for this and the next reference to the kindness of the Manchester City Librarian.

[4] W. A. Abram, *History of Blackburn*, 1877, p. 225. In other references cited below a number of variants of these spellings occur—Livesy or Livesay for Livesey; Hargrave or Hargreave for Hargreaves; Anstead for Anstie. I do not know which is correct.

[5] *Collis and Others v. Emett* (1 H. Bla. 313). See below. I am grateful to Mr. M. H. Longfield for opportunities to consult the Law Reports here cited.

[6] June 17th, 1788, p. 299.

Curiously enough it was one of their accomplices, and not they themselves, whose name stuck in contemporary memories. The passage from Sir Francis Baring, to which reference has already been made, reads as follows :

> "I believe there was no material convulsion, with regard to circulation, from the Rebellion, until that which happened by the failure of the Air Bank, in the year 1772. . . . The next check to commercial credit arose from the failure of a circulation established between Lancashire and London, well known in the courts of law by the case of Gibson *v.* Johnson, which has been so often tried."[1]

A little later, harking back to the same subject, he adds :

> "The circulations . . . from Lancashire, mentioned before, were, I think, to the amount of about two millions."[2]

In point of fact, there is no case called "*Gibson v. Johnson*" which can possibly be the one Baring had in mind. He seems to have been referring to one cited as *Minet v. Gibson*,[3] the full title being *Minet and Fector v. Gibson and Johnson*, which was heard in the King's Bench on November 3rd, 1789, went to the House of Lords in April, 1790, and was finally decided in February, 1791. It concerned a bill of exchange drawn by Livesey and Co. as from Manchester, and accepted in London, on February 18th, 1788, by Gibson and Johnson, bankers. The identity of this case with the one mentioned by Baring is supported by the concluding words of the report of it in the *Gentlemen's Magazine*[4] : ". . . the judgment of the Court of King's Bench is affirmed, which entitles the holders of these bills to the amount of upwards of one million sterling to recover."

Just why Baring mis-cited it we can only surmise, but there is a passage in another contemporary case, *Tatlock v. Harris*,[5] which seems to show that Messrs. Gibson and Johnson had acquired a certain notoriety. In that case the plaintiffs called a witness, Benjamin Fuller, who testified that "he had heard the defendant say . . . that if certain persons, called Gibson and Johnson, and other persons, on whom fictitious bills of exchange were drawn, should pay, he the defendant would pay . . . but that he the defendant would not pay the same for the present, and till the matter was decided." This conversation passed "a day or two before" 6th July, 1788. Since the bill in *Minet v. Gibson*, which fell due on 18th May, 1788, remained unpaid, it is clear that, at least a month before the bankruptcy, Gibson and Johnson had had warning that Livesey and Co. were toppling, and were seeking to escape from their own commitments.

One can hardly blame them for that, for neither at the time they accepted the bill on which they were sued, nor at any time since, had they had "any money, goods or effects whatever, of or belonging to the said Livesy, Hargrave and Co., or of the said Hughes Minet and James Peter Fector in their hands". So the Special Jury in November, 1789. They were, in fact, merely lending their name for a commission. But the scale of their operations, together with their pertinacity in taking their opponents to the House of Lords, led to a legal wrangle of a magnitude which was evidently fresh in the minds of the commercial world in 1797.

The points which Gibson and Johnson elected to fight, and which were legally established by the Lords' decision, are summarised by McLeod as follows : (*a*) If the drawer of a bill inserts the name of a fictitious payee and the acceptor knew that the payee was fictitious, the holder may sue him on it as payable to bearer ; (*b*) if the bill is drawn in a fictitious name or is a forgery of a real name, to the knowledge of the acceptor, he undertakes to pay to an indorsement by the same hand.[6] Something very like them had been encountered by Lord Mansfield in *Stone v. Freeland* twenty years earlier,[1] but he had then deemed it sufficient to rule that the acceptor could not be heard to deny that an indorsement was genuine if he had been privy with the drawer to the creation of a bill merely to enable the drawer to raise money.

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[1] *Observations* . . . . p. 14. These sentences are repeated almost word for word, without acknowledgment, by Lawson, *History of Banking*, p. 267.

[2] *Ibid.*, p. 33.

[3] 3 Term Rep. B.R. 481. 1 H. Bla. 569.

[4] Vol. LXI, Part I (1791), p. 176 *et seq.*

[5] 3 Term Rep. B.R. 174. See below, p. 235.

[6] *Theory and Practice of Banking*, 3rd Edn., Vol. II, pp. 513, 520.

More formally, the pleas first arose, in the cases in which we are now interested, in *Tatlock v. Harris*, already cited. Messrs. Harris, Harris and Plant, of Nottingham, drew on Harris, a partner, at his London address, a bill in favour of Grigson and Co., an imaginary firm, and endorsed it, in that name, to Lewis and Potter, who in turn transferred it, for full consideration, to the plaintiff Tatlock.[2] Subsequently, as we have seen, Harris declined to pay, until Gibson and Johnson should do so on similar bills, or until their test case had been fought. But for some reason *Tatlock v. Harris* came on for hearing in May, 1789, six months before *Minet v. Gibson* reached the Courts, so that Harris had to make the running. On his behalf, therefore, we find the arguments which the plaintiffs in all such cases had to meet. As was to be expected where equity evidently lay with the plaintiffs, the defendant took the strictly legal line. He argued, in effect, that a *chose in action* being only assignable under strictly limited conditions, the appropriate rules must be rigidly complied with. For bills of exchange drawn to order, these rules required that the handwriting of endorsements must be proved, and the plaintiffs had produced no evidence that Grigson and Co. had endorsed the bill. Alternatively, he claimed that the endorsement was forged, and that this invalidated all claims under the bill. In response, counsel for the plaintiffs contended that as Grigson and Co. were non-existent to the knowledge of the defendant, the bill must be considered as payable either to Lewis and Potter, the next payees, or to bearer. Other arguments concerned alternative counts for money had and received. For the defendant it was contended that these could not succeed because there was no direct privity of contract between acceptor and holder. This the plaintiffs denied; the acceptor was obliged to pay to the bona-fide holder, and must therefore have privity of contract with him. In any case, the defendant in this case was drawer as well as acceptor and could be sued in that capacity as well.

The Court evidently brought to the case a feeling that, whatever the legal position, this was a case where somehow justice must be done (this attitude is strikingly apparent throughout the whole series of judgments in the cases now under review). In the event, they held that the defendant had effected an appropriation of the amount of the bill, to be paid to the person who became the holder of the bill. This was tantamount to treating the bill as one to bearer, for the decision rested on *Grant v. Vaughan*,[3] where a similar appropriation was inferred in the case of a bearer bill.

On the day that *Tatlock v. Harris* was decided a similar case was heard which again involved Livesey and Co. This was *Vere and Others v. Lewis and Another*[4] which concerned a bill drawn by Livesey and Co. on the defendants in favour of Lawrence Ashworth, another imaginary payee, and then endorsed to the plaintiffs. Counsel for the defendants tried to distinguish this from *Tatlock v. Harris* on the grounds that there was no evidence that the defendants had received any consideration for the bill, but the Court held that the mere fact of their accepting it was evidence that they had received value for it. Judgment for the plaintiffs as before.

When, therefore, *Minet v. Gibson* reached the lower Courts in November, 1789, there was nothing fresh to decide, and it was rapidly embodied in a special verdict so that it might go forward to the Lords. Once more there was a fictitious payee—this time Mr. John White—and this time the Court was presented with a series of counts, in the form of alternative hypotheses as to what must be deemed to have happened when Livesey and Co. wrote the name of John White upon the bill. Thus, the second count recited that Livesey and Co. used the name of J. White as a nominal person only, not intending to deliver the bill to him; the third, that it was made payable to themselves (Livesey and Co.) by the name and description of J. White; the seventh, that the persons comprising the firm of Livesey and Co. also comprised the firm of J. White, and in that capacity indorsed the bill; the fourth (leaving nothing to chance) that J. White himself indorsed it. The fifth and sixth contended respectively that the bill either was, or should be regarded as, payable to bearer. On the rule established by *Tatlock v. Harris*, judgment was given for the plaintiffs on the fifth count.

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[1] (1769.) For a note of this case see 1 H. Bla., 316n.

[2] This was a common practice. See, e.g., Ashton, *An Eighteenth Century Industrialist*, esp. p. 104. I am indebted to Mr. P. B. Whale for drawing my attention to the interesting sidelights this book throws on these problems.

[3] 3 Burr. 1516.

[4] 3 Term Rep. B.R. 182.

And so finally the battle is joined. A writ of error is brought, returnable to Parliament, and the plaintiffs in error having assigned general errors, and the defendants in error having pleaded that there was no error in the records and proceedings, both sides set out their reasons, and their Lordships hear great argument. Meantime all parties have followed Livesey and Co. into the Bankruptcy Court, which prevents the plaintiffs from pursuing any alternative common law remedy which they might have had. The case must be fought on the bill, and since, if we may believe the reporter, " bills to the value of near a million a year " had passed through these houses alone,[1] the stakes are worth fighting for.

Eventually the six judges deliver their views; four find for the plaintiffs, and two for the defendants. But Minet and Fector are not yet out of the wood, for the four have expressed the opinion that the bill is both one to bearer and one to order, and this is too much for the Lord Chancellor. " He was extremely glad this subject had come to be heard before persons whose experience, knowledge and practice in the law, must command their lordships' attention and acquiescence. For his part, he was ready to say, if those persons pronounced any thing to be law which he could comprehend, he should find himself disposed to obey it. He did not see how he could well go further . . ."[2] But he really could not see how a bill could simultaneously be payable both to order and to bearer. " It would be proper, therefore, that the four learned Judges should make an election."[3]

However, eleven days later all is well. The subject stands part of the order of the day in the House of Lords, and although the Lord Chancellor speaks against the majority verdict, he is out-argued by Lord Kenyon (Chief Justice), Lord Loughborough, and Lord Bathurst. The first two of these had already decided the analogous cases, the one in King's Bench and the other in Common Pleas, and they now re-stated the substance of their judgments. The House is convinced, and without a division the opinion of the majority is sustained. Minet and Fector have won; though what resources Gibson and Johnson have by now from which to pay their million pounds, history does not record.

. . . .

We may briefly ask ourselves, in conclusion, what is the economic significance of this protracted drama. In the first place, it is clear that public opinion was gravely concerned at the widespread use of accommodation bills. The pamphleteer was at work—see, for instance, Godfrey's *The Great Question Considered Concerning Fictitious Bills.*[4] The dissenting judges in the House of Lords denounced the evil, indeed, even more forcibly than the majority. Thus, Mr. Justice Heath remarked:

> " It is agreed on all hands, that the circulation of these bills is extremely mischievous, and ought to be restrained. It is the great commercial evil of these days, which has grown to a gigantic height. It has enabled needy adventurers to engage in desperate undertakings, relying on the money which they raise on this fictitious credit . . . this traffic has spread poverty, distress and bankruptcy, through large districts which it has pervaded."[5]

Lord Chief Baron Eyre spoke of

> " that false coinage of base paper money, which has been of late forced into circulation; the use of which is to encourage a spirit of rash adventure, a spirit of monopoly, a spirit of gaming in commerce, luxury, extravagance and fraud of every kind."[6]

He considered this was " a fortunate occasion to check a growing evil ". But whereas these two learned judges believed that the cure lay in refusing to enforce recovery on any bill which was not demonstrably sound, the majority considered that holders in good faith should not be penalised because of defects in the bill of which they were not aware; and that the use of such paper could be better discouraged by preventing evasion of the consequences. In this opinion they were patently right. But that such strong concern should be felt, even in the seclusion of the House of Lords, over contemporary commercial morality throws a lurid light on the get-rich-quick methods of the early Industrial Revolution.

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[1] Editor's footnote to *Minet v. Gibson* in 3 Term Rep. B.R.

[2] Quoted from the *Gentlemen's Magazine, loc. cit.*

[3] *Ibid.*

[4] Noticed in the *Gentlemen's Magazine* for 1791 (Vol. I, p. 376). I have not been able to obtain access to a copy.

[5] 1 H. Bla. 624.

[6] 1 H. Bla., 619.

In the second place, it would seem from the expressions used by Baring that the crop of such bills in the 1780's arose from an interchange between Lancashire manufacturers and London financial houses. That others than the actual parties in *Minet v. Gibson* were concerned is obvious, not only from *obiter dicta* throughout the hearings, but by the differing names appearing, e.g. in *Vere v. Lewis*. If the interruption of this interchange caused as intense a shock as the sums involved would suggest, what happened to the currency of Lancashire after 1788—and especially during the 2¾ years that the legal position remained obscure ?[1]

Finally we may see in these events an additional source of the public nervousness which, aggravated by doubts about French intentions, led soon afterwards to a sudden attack on the country banks, a consequent demand for liquidity on their part, and the crisis of 1793. It is customary to assert that this crisis was due not to speculation but to the over-expansion of legitimate credit,[2] but if the interpretation here suggested is accurate, the issue of Exchequer Bills may have offered a double relief. Not only will they have provided additional liquidity for hard-pressed bankers, but they may also have enabled industrialists, accustomed to making payments in their own bills, to offer a more acceptable medium to creditors still suspicious of all privately-created paper.

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[1] I am obliged to pose this question and not to answer it, because the extremely restricted facilities for research now available to those whose work ties them to London have precluded any further examination of the problem. It seems, however, to be worth looking into, although on the purely negative evidence of an absence of contemporary clamour there may have been no serious dislocation. Ashton's study, already referred to, gives no instance of credit difficulties *circa* 1790, though several are quoted for 1793.

[2] E.g. Feaveryear, *The Pound Sterling*, p. 165.

# Permanent Resources

By G. F. THIRLBY

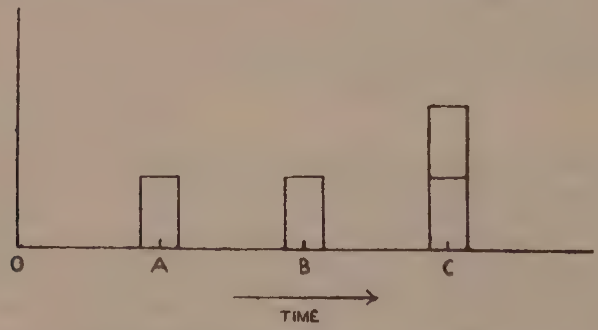
CERTAIN passages in Professor Hayek's recently published work[1] suggested a dilemma, leaving a choice between a concept of permanent resources and an alternative concept of non-anticipatable returns.[2] It seemed possible that the dilemma might be removed if the concept of " resource " were broken down into two elements which I have tentatively named " non-cumulable " and " cumulable " units of service, either of which might or might not possess the quality of being what I have called " quasi-cumulable ". An attempt to perform this analysis led me to a concept for which I have, rightly or wrongly, retained the term " permanent resource ". This might, I suppose, be said to be a two-dimensional concept in that it implies, not only the quality of lasting throughout the relevant future, but also that of adequacy at any relevant point of time. It appears to have the advantage of subsuming both those situations in which it is, and those situations in which it is not, possible to " anticipate returns ".

The analysis and definition are given in Section I below. In Section II the discussion is related to some statements by Professor Hayek. Section III is devoted to an issue on " depreciation " raised by Mr. Hawtrey.

It is perhaps advisable to emphasise at the outset that the discussion, in all three sections, never moves beyond a single *ex ante* decision: it does not, for example, consider what might happen to the " maximum physical capacity " of the resource, and to the cumulability and time-dispersion of the units of service if, after beginning operations in accordance with his *ex ante* plan, the decision-maker did, as he might be expected to do, rebudget—possibly at the same time advancing the termination of the period in which he was interested to a point of time further ahead.

## I

Let us suppose that the maximum physical capacity of a resource[3] to give rise to physical services may be represented by a time-series showing the dispersion of the capacity over time. Thus



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[1] *The Pure Theory of Capital* (Macmillan, 1941).

[2] " It is evident that certain resources may have to be treated as permanent in this sense, because their expected future services cannot be sacrificed in order to increase satisfaction in an earlier period, but will have to be treated as non-permanent in the other sense because their services, once they become available, are non-recurrent." (*Op. cit.*, p. 52, footnote.)

[3] I should define " resource " as whatever, in the state in which he expects it to exist at the beginning of the budgetary period, and after rejecting alternative opportunities for its disposal, the decision-maker reserves for the production of the particular service in question; i.e., everything which, in that anticipated state, he deems to be within the production unit at the outset. And I wish it to be assumed that, at that point of time, everything is (planned to be) within one production unit or another, and nothing is (planned to be) in two production units at once; i.e., each production unit contains one resource, and the total number of resources equals the total number of production units.

*A*, *B*, *C* are three points of time. The height of the three blocks over *A*, *B* and *C* together represent the maximum total physical services that it is possible to get out of the resource, and the separate heights represent the dispersion of that maximum amount over time. Let the block at *C* be divided into a number of units, say two. Then the blocks at *A* and *B* each represent one unit. We may say that the maximum physical capacity of the resource is four units of service, dispersed over time as indicated in the graph.

It is conceivable that a unit of service of the resource might be capable of becoming available at a point, or perhaps several alternative points, of time other than that at which it occurs in the time-series, without reducing the maximum physical capacity. I shall speak of a unit which possesses this quality as a unit which is capable of becoming *fully* available at a point of time other than that at which it occurs in the time-series, or as a unit which is equally capable of becoming *fully* available at more than one point of time. This possibility of the unit's being capable of becoming fully available at another point of time is to be taken to mean that there is another alternative dispersion (an adjusted time-series) which also represents the maximum physical capacity of the resource to give rise to physical services. (I shall generally ignore the possibility that, if the unit were divisible, it might, in the second alternative dispersion, itself be dispersed over a number of points of time.)

The unit of service which is thus equally capable of becoming fully available at more than one point of time might be called cumulable, and be said to be cumulable at any one of those points of time. The unit is not to be conceived of as one which *emerges* necessarily at one point of time, and whose availability at another point of time consequently involves manipulative *transfer* to prevent its loss. What is given is that it is equally capable of becoming fully available at any one of the alternative points of time without any such manipulative action being taken at all.[1] In the appropriate *ex ante* form the definition of the unit would run: a unit of service which is conceived *ex ante* by the decision-maker to be capable of becoming fully available, at his will, at either of two, or any one of more than two, points of time. From the viewpoint of the decision-maker standing at the budgeting point of time prior to the points of time included in the period for which he is budgeting, the unit of service in question is observed merely as a unit which is cumulable, at his will, at any one of two or more points of time, the act of cumulation, as such, involving no cost to him beyond such cost as is involved in the mental process of budgeting. The unit's becoming fully available at one point of time or another may be regarded as an event whose occurrence is determined, at the will of the decision-maker, at one of those points of time or another.

It is also conceivable that a unit of service may be *in*capable of becoming fully available, in the sense explained above, at any point of time other than the one at which it occurs in the original time-series representing maximum capacity. Such a unit may be called a non-cumulable unit. No dispersion of the occurrence of non-cumulable units could be arranged, other than that in the given time-series, which would allow maximum capacity of the resource. If the services of the resource consisted entirely of non-cumulable units, there would be only one dispersion of units consistent with maximum physical capacity. A non-cumulable unit of service may be defined as a unit of service which is conceived *ex ante* by the decision-maker to be capable of becoming fully available at one point of time only.

Clearly, a unit cannot be capable of becoming *more* fully available, in the sense referred to above, than it is at the point of time at which it occurs in a time-series representing maximum physical capacity of the resource; if it could, a redispersion of units could achieve a physical capacity higher than the maximum.

We may now say that any time-series representing maximum physical capacity of the resource shows a dispersion of the total number of units of service of the resource every one of which is capable of becoming fully available at the point of time at which it occurs in the series, and every one of which is either a cumulable or a non-cumulable unit.

As all units which occur at a particular point of time in a time series representing maximum physical capacity of the resource are capable of becoming fully available at that point of time, they may eventually *emerge* at that point of time. If they are cumulable units, their emergence at that point of time depends upon the decision of the decision-maker. If they are non-cumulable units, emergence may depend upon his decision, or it may occur without his interference, or even without his being able to prevent it. Emergence which fits the decision-maker's production plan, even though he may not have been able to prevent the emergence, I shall speak of as *decumulation.* Emergence which does not fit the decision-maker's plan (in loose language, emergence which occurs although the decision-maker does not want it to do so) is not to be regarded as decumulation: the verb " to leave undecumulated " which I have used below should be interpreted as meaning either to leave unemerged or *to allow to run to waste.*

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[1] Cf. Professor Hayek's statement concerning non-anticipatable returns: "All that is necessary is that it should be expected to be useful, not in consequence of being kept in that state at a sacrifice, but because no present advantage would arise from destroying its future usefulness". (*Op. cit.*, p. 52, footnote.)

The possibility of emergence, and consequently of decumulation, may be referred to as decumulability. We may say, then, that all cumulable and non-cumulable units of service are (*ex ante*, expected to be) *fully decumulable* at the points of time at which they are represented in a time-series representing maximum physical capacity of the resource.

It is now necessary to define quasi-cumulability. It is conceivable that either a cumulable unit or a non-cumulable unit may be capable of becoming *partly* available at a point of time different from its point of time of full decumulability. " Partly available " is intended to indicate that occurrence at the point (or points) of time other than the point of time of full decumulability would involve some net destruction of the unit itself or some other unit or units so that the total availability of all units of service together would be reduced, i.e., the adjusted time-series would represent a position of submaximum physical capacity of the resource. If, for example, two non-cumulable units occurred at *A*, two half-units (= one unit) might be expected to be capable of becoming available at *B*, the remaining two half-units (= one unit) being expected to be destroyed by the actual decumulation at *B*, or by the failure to decumulate at *A*, the possibility of decumulating one unit at *B* being (expected to be) contingent upon decumulating neither unit at *A*. Similarly, if two cumulable units were cumulable at either *A* or *B*, but not at a third point of time, *C*, two half-units (= one unit) might be expected to be capable of becoming available at *C*, the remaining two half-units (= one unit) being expected to be destroyed by the decumulation at *C*, or by failure to decumulate at either *A* or *B*, the possibility of decumulating one unit at *C* being (expected to be) contingent upon decumulating neither unit at either *A* or *B*. Thus, any unit, cumulable or non-cumulable, may, or may not, be quasi-cumulable as well.

A production plan which arranged a dispersion of units only at points of time at which they were fully decumulable might be referred to as a production plan *consistent with maximum physical decumulation.* If the resource consisted of units which included cumulable units, there could conceivably be many variations of the production plan, involving different dispersions of the cumulable units, all consistent with maximum physical decumulation. Let us first suppose that the decision-maker was under an obligation to decumulate and dispose of the *whole* of the units of the resource, i.e., to use the resource to maximum physical capacity and dispose of the whole result, but that otherwise the particular dispersion chosen depended upon the anticipated state of the market[1] at the various points of time which the budgetary period covered. Subject to the stipulation that all units must be decumulated and disposed of without destruction, that dispersion would be planned which would be expected to lead to a decumulation which would maximise the decision-maker's returns[2] from the disposal of the decumulated units. Such a planned decumulation might be referred to as the *optimum maximum physical decumulation.*

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[1] The term " state of the market " should be interpreted broadly to include those situations in which we have to deal " either with the economy of an isolated individual, or with that of a communist society where all activity is directed by a dictator ". (Hayek: *op. cit.*, p. 99.)

[2] The expressions " maximise the decision-maker's returns " and " restrict output " which I have been led to adopt through my greater familiarity with analysis which uses such phrases) should not be allowed to give the impression that I am necessarily contemplating a situation in which the decision-maker is aiming at the maximisation of monopoly net revenue. Rather than attempt to elaborate the possible meaning of the expression " restrict output ", I prefer to rely upon the following statement made by Professor Hayek: " In general, the reason why resources which are capable of being turned to some useful purpose are not actually so used is that they would have to be combined with other resources which are more urgently needed elsewhere. So long as these other, complementary, resources cannot be spared because the total quantity of them available is required for purposes where they will yield a greater return than they would yield in co-operation with the potential resource in question, this resource will remain unused, or latent . . ." (*Op. cit.*, pp. 61 and 62.) If this can be the reason for the lying unused of a whole resource, I presume it can be the reason for the lying unused (or restriction of output) of part of the services of a resource.

I am under the impression, though I am not certain, that my " permanent resource " corresponds closely to Professor Hayek's " latent " or " potential " resource which is " used " though it has not become " scarce ". [" Of those [potential or latent resources] actually used only a part will become scarce "—*op. cit.*, p. 61.) But the resources which he conceives of as becoming " scarce " while remaining " permanent " would be either quasi-permanent or non-permanent in the sense in which I have used these terms.

If it were *not* stipulated that all units must be *decumulated and disposed of*, although it were still stipulated that the *dispersion* must be one *consistent with maximum physical decumulation*, then the decision-maker, aiming at the maximisation of returns, would not be prohibited from planning to leave some units undecumulated, i.e., he would be free to plan to restrict output in order to maximise returns. A planned decumulation under these conditions might be called the *optimum decumulation within a dispersion consistent with maximum physical decumulation.*

If the stipulation that the dispersion must be consistent with maximum physical decumulation were removed also, the decision-maker[1] might plan a completely different dispersion which involved quasi-cumulation, and consequently a decumulation which for this reason involved the destruction of some units. He might *also* plan to leave some units undecumulated (restrict output). Let us call a decumulation planned in these circumstances the *optimum decumulation.*

We might then describe the resource as a permanent one if, according to the plan for optimum decumulation, for every point of time within the budgetary period, either a non-cumulable unit occurring at that point of time, or a cumulable unit cumulable at that point of time, or a quasi-cumulable unit quasi-cumulable at that point of time, were planned to be left undecumulated, or if, while actually not occurring, such a unit would, if it could be supposed to occur, be planned to be left undecumulated. In other words, for the resource to be a permanent one, either at every point of time, services capable of becoming available at that point of time must be planned to be left undecumulated after the planned maximisation of returns from the disposal of services of the resource, or, if at any point of time services are not so planned to be left undecumulated, the returns from the disposal of services of the resource must not be potentially capable of being increased by the decumulation and disposal of a further (new) unit that might be supposed to occur. In loose language, there must, at every point of time, be either " too many " or " just enough " services capable of becoming available. These services (including in the appropriate context the non-existent ones that would be left undecumulated if they existed) which must be planned to be left undecumulated if the resource is to be regarded as a permanent one, are referred to for the sake of brevity in the following discussion as " *the surplus units* ".

It is conceivable that, although the plan for optimum decumulation of the units of service of a resource did not disclose " the surplus units ", it might none the less disclose surplus units (understood in the same sense) at *some* points of time. Such a resource might be referred to as a *quasi-permanent resource*, the term *non-permanent resource* being used to refer to a resource with no surplus units at any point of time. To shorten the discussion, I shall assume that there are no quasi-permanent resources, but that there are permanent and non-permanent resources.

The non-permanent resource requires further consideration. It will be apparent that to assume that the decision-maker would plan merely to decumulate it according to the plan for its optimum decumulation, would be implicitly to posit an intolerable state of disequilibrium. The absence of " the surplus units " indicates that if, in some way, further (new) decumulable units could be made to occur at the appropriate points of time, returns might be increased further. The increase cannot be achieved from the resource as it is expected to exist at the outset, but it might conceivably be achieved by adding something to the resource which would, as it were, fill in the deficiencies at the appropriate points of time during the budgetary period. There appears to be an incentive to the decision-maker to do this; equilibrium throughout the budgetary period seems to demand that he should do so; the planned decumulations of other production-units provide the possibility of doing it.

But with the possibility of adding something to the resource in this way, the planning of the decumulation of the resource itself is only a part of the decision-maker's whole plan for the particular production unit. The time-series representing his optimum plan of decumulation for the whole budgetary period will include units which can occur at particular points of time only because he plans acquisitions of the decumulations of other production units. In this full plan " the surplus units " will appear: anticipated returns will be maximised. But the appearance of " the surplus units " will not have converted the resource into a permanent one, because they could not have been made to appear in a time-series representing the optimum plan[1] of decumulation of the resource alone.

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[1] Aiming at maximising returns.

The question now arises whether all non-permanent resources have to be regarded as capital. It is possible that the answer might be found by examining the nature of the resource's units of service in relation to the markets for those services. The decision as to whether we do or do not have to regard a particular unit of service as capital might be said to depend upon whether or not the optimum plan for the decumulation of the resource alone[2] would or would not include a " charge ", of the opportunity cost type, against the unit for " depreciation "[3], where the alternative opportunities giving rise to the " charge " are *either* contemporaneous alternatives, *or* inter-temporal alternatives, and are opportunities of different time-orders—different distances in time from final income—one of which may be the limiting case of final income.[4] The units which would be " charged " with this " depreciation ", and which should therefore be regarded as capital-units, would consist of all cumulable and quasi-cumulable units with either a contemporaneous or an inter-temporal opportunity, and all remaining non-cumulable units with a contemporaneous opportunity, of decumulation for an investment of some other time-order. These would exclude any unit which was for any reason " specific " to an investment of any particular time-order in the sense that no such alternative opportunity existed.

## II

It is now possible to relate the concepts to certain statements made by Professor Hayek. To do so, it will be convenient to make use of a very simple model similar to the time-series used in Section I.

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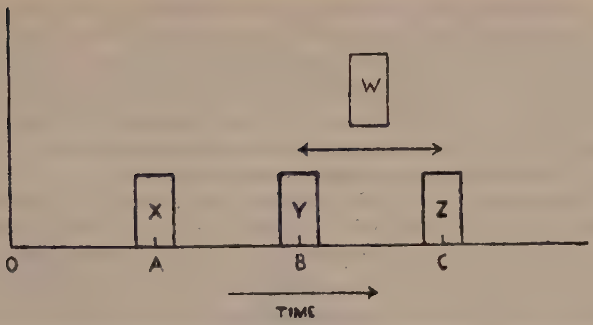
[1] I confess to being in doubt as to whether assumptions about the planned acquisitions and decumulations of *other* production units have to be different, for the drawing up of this partial plan, from the assumptions made for the drawing up of the plan for the full production unit. If the assumptions are different the conditions of the market for the production unit's decumulations (e.g.) may be different, and may alter the optimum time-dispersion of units of service.

[2] It will be observed that, if it is true that in equilibrium " the surplus units " appear in the time-dispersion of a production unit which *ex ante* consists of a non-permanent resource, there is no possibility of the emergence of any " depreciation charge ", against the planned decumulations of the production unit, of the type regarded as the criterion for determining what is capital. The reason for this is that " the surplus units " indicate that at every point of time in the time-series the units occurring are adequate (to maximise returns) *for each possible opportunity considered separately* : no alternative revenue of any kind is sacrificed by fully exploiting an opportunity for investment of any particular time-order. What we have to do, apparently, in order to discover what is capital and what is not, is to consider the optimum decumulation of the resource alone, without the planned additions, and discover what " depreciation charges ", of the type which is the criterion, *would* be made if nothing were planned to be added to the resource. (I have suggested this criterion with the same doubt in my mind as that which prompted the previous footnote.)

[3] The meaning of a " depreciation charge " of the opportunity cost type is, I hope, made clear in Section III.

[4] To state the criterion in this way incidentally rules out any effect of *mere* time-preference as a determinant of what is and what is not capital. For example, if both of two inter-temporal alternative opportunities were regarded as opportunities for decumulating the units as final income, then the " depreciation charge " which was a determinant of the relative amounts planned to be decumulated at the two points of time would *not* be a criterion indicating that the units planned to be decumulated were capital. The " depreciation charge " would indicate a *mere* time-preference. Similarly, if the two inter-temporal opportunities were opportunities for investments of the same time-order, other than final income, the " depreciation charge " would be no criterion. (Such a mere time-preference " depreciation charge " can, of course, exist only if the units concerned are cumulable or quasi-cumulable—*vide* Section III.) But if one of the two opportunities were for decumulation for, say, final income, and the other for an investment of a particular time-order of some distance from final income, the existence of the " depreciation charge " would be the criterion : it would indicate a planning of a division of the units between final income and investment of the particular time-order ; the unit would be capital. Similarly, if the two opportunities were *contemporaneous* opportunities for investments of different time-orders, the unit would be capital.



*O* is the point of time at which the decision-maker budgets, or makes his decisions.

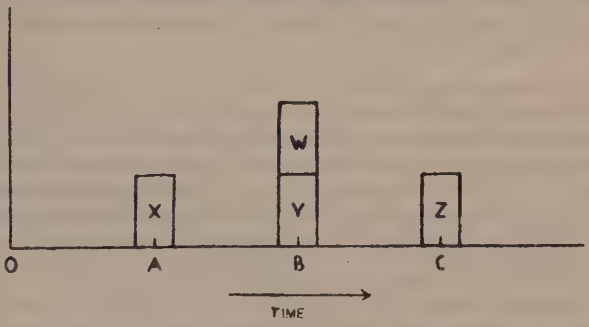
*AC* is the budgetary period, containing only the three points of time *A*, *B* and *C*, *A* and *C* being the limits of the period in which the decision-maker is interested.

*X*, *Y*, *Z* are three non-cumulable units of service, fully capable of becoming available, and being fully decumulable, only at their respective points of time *A*, *B* and *C*.

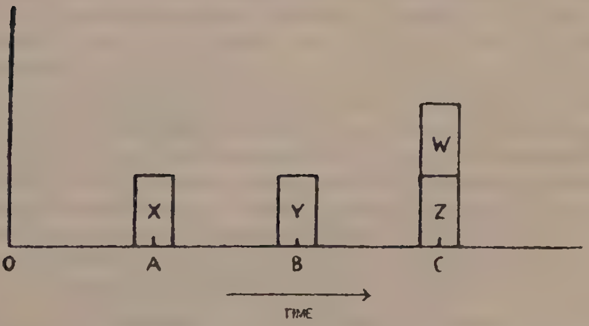
*W* is a single cumulable unit of service, cumulable and fully decumulable at *B* or *C*, but not at *A*.

If the decision-maker intends to achieve maximum physical decumulation, his plan for decumulation must be one of two alternatives:—

(1)



or (2)



I. " What may be regarded as an even more fundamental basis for the distinction [between permanent and non-permanent resources] is the fact that the future services of some resources cannot be anticipated, as they will continue to give the same services in the future no matter how they are used in the present, while the present use of the services of other resources decreases the amount of such services which will be available in the future." (*Op. cit.*, p. 52.)

If, in the above model, the units of service consisted of only *X*, *Y* and *Z*, and if none of them was quasi-cumulable, the *resource* from which these three non-cumulable units arose would be a resource of the first type referred to in this statement (" future " being interpreted as referring to a later, and " the present " being interpreted as referring to an earlier, point of time in the budgetary period). The impossibility of anticipation of use would depend upon the absence of cumulable or quasi-cumulable units of service in the relevant period; the certainty of continuance of undiminished (expected) physical yield further ahead, however intensive the earlier use, would depend upon the presence further ahead of non-cumulable units which were also not quasi-cumulable. Obviously this type of " permanence " depends upon the occurrence in the " future " in at least some degree, of units which are non-cumulable, and which are not quasi-cumulable. But the resource would still not be permanent according to the definition of Section A, unless the budget disclosed " the surplus units ".

But if, in the model, the unit $W$ were present under the conditions originally assumed, then the resource could, for the period $BC$, be regarded as an instance of the second type referred to in the statement. If $W$ were cumulated and (planned to be) decumulated at $B$, instead of at C, the amount of services (planned to be) available at $C$ would be reduced from $Z+W$ to $Z$. If $Z$ were quasi-cumulable, its quasi-cumulability would, in the appropriate anticipated conditions of the market, allow a similar effect of reducing the amount (planned to be) available at $C$. The decrease, by (planned) earlier intensive use, of (planned) yield further ahead depends upon the presence of cumulable or quasi-cumulable units in the relevant period. A more extreme example of this type would obviously be provided by assuming that all units of service were cumulable at any point of time included in the whole budgetary period. But these qualities of the units would not necessarily make the resource non-permanent according to the definition of Section A, because it is conceivable that the budget might still disclose " the surplus units ".

> II. " The underlying fact, and in a sense the most general aspect of the phenomenon under consideration, is the *irreversability of time* which puts the future services of certain resources beyond our reach in the present and so makes it impossible to anticipate their use, whereas the present services of those resources can as a rule be postponed." (*Op. cit*., p. 52.)

And

> ". . . the irreversability of time, i.e., the fact that we can postpone but not anticipate the use of certain resources . . ." (*Op. cit*., p. 53, footnote.)

It is perhaps worth while to use the concepts of cumulable, non-cumulable and quasi-cumulable units of service for the purpose of analysing further these notions of the irreversability of time and anticipation and postponement of services. Our adoption of the *ex ante* viewpoint strictly prevents our speaking of postponement from and anticipation to " the present ": in using the expression we shall really be referring to postponement from and anticipation to a particular point of time relatively early, say the first point of time, in the budgetary period.

On the assumption that no units at all are quasi-cumulable, it would appear to be impossible that the irreversability of time should, while allowing postponement from any point of time (" the present ") prevent anticipation at that *same point* of time. If all the units are non-cumulable units, both postponement and anticipation are impossible according to the definition. But if any units are cumulable units, the possibility of postponement involves an equivalent possibility of anticipation, and *vice versa*. For example, at $B$, in the model, it is possible to postpone the use of the cumulable unit $W$ to $C$, by adopting plan for decumulation (2)—above—instead of plan for decumulation (1). But at the same point of time, and to exactly the same extent, anticipation is possible: it is possible to anticipate the use of the same cumulable unit $W$, by adopting the alternative plan for decumulation (1) instead of plan (2). In other words, it is possible to regard cumulable units of service as both " future " to (after) a certain point of time and " present " at that same point of time. It seems to be desirable, therefore, to distinguish (conceptually) between the " future services " of a resource which are " beyond our reach in the present " and the " present services . . . [which] . . . can . . . be postponed ". Quasi-cumulability apart, the former are non-cumulable units decumulable in " the future ", and cumulable units whose earliest date of cumulability is later than " the present "; the latter are cumulable units which, in order to be " *present* services ", must be cumulable as early as " the present ", and, in order to be cumulable as well, must be anticipatable from " the future " as well as postponable from " the present ". At $A$, in the model, the non-cumulable units $Y$ and $Z$ are " beyond our reach in the present ": it is " impossible to anticipate their use ". This is true also of the cumulable unit $W$, because it is not cumulable as far back as $A$. But if we assume that $W$ is cumulable at $A$ as well as at $B$ and $C$, the unit $W$ becomes " present services . . . [which] . . . can . . . be postponed " to $B$ or $C$, but it equally becomes " future services " which can be anticipated from $B$ or $C$. The " underlying fact " which Professor Hayek refers to as " the irreversability of time " might perhaps be better referred to, so long as we continue to exclude quasi-cumulability, as the fixity (or specificity) *in* time of the non-cumulable unit and the non-fixity (or non-specificity) *in* time of the cumulable unit. The concepts would then apparently fit his statement if it ran: " The underlying facts . . . are that of the fixity at some future point of time, or the non-anticipatability until some future point of time of some of the services of certain resources, which puts them beyond our reach in the present and so makes it impossible to anticipate their use, and the fact that other services of those resources can as a rule (?)[1] be made to occur in the present or in the future and can thus be postponed to the future ". Some units of service are either fixed at a relatively distant point of time, or anticipatable (and equally postponable) only when some relatively distant point of time is reached; other units of service are anticipatable (and equally postponable) when some relatively early point of time is reached.

The quasi-cumulable unit is, as we have seen, one which is expected to diminish the resource's capacity to yield services if an attempt is made to decumulate it at a point (or points) of time other than that at which it occurs in a time-series (expected to be) consistent with maximum physical decumulation. So long as, in conformity with the definition, the result were expected to be a net reduction of total physical capacity of the resource to give rise to physical services, the total result might conceivably include various increases and decreases of capacity at various points of time earlier and later than the point (or points) of time at which the unit in question were capable of becoming fully available. But let us concentrate our attention upon a quasi-cumulable unit which is expected, if an attempt is made to decumulate it at a point of time other than that at which it is fully decumulable, to *increase* the occurrence of decumulable service (by, say, half of a unit) only, if the quasi-cumulable unit is a non-cumulable unit, at earlier points of time than the one at which it is fully decumulable, and only, if it is a cumulable unit, at earlier points of time than the earliest of an uninterrupted sequence of points of time at which it is fully decumulable, and to *decrease* the occurrence of decumulable service (by the full unit) only at the point or points of time at which it is fully decumulable. It then appears to be true that anticipation at a certain point of time will (be expected to) result in a decrease of capacity, and postponement from that same point of time in an increase of capacity. But it does not appear to be true that there are any services the use of which can be postponed but not anticipated. If, in the model, the non-cumulable unit $Y$ were (expected to be) capable of decumulation at $A$ as half-a-unit, instead of at $B$ as a full unit, this being the only possible effect of quasi-decumulation, then the use of the half-unit of service which is as much of $Y$ as could be *postponed*[2] from $A$, because it is as much of it as can be decumulated at $A$, could also be anticipated at $A$. But postponement would result in increased total capacity whereas anticipation would result in decreased total capacity. It is, however, true that $Y$ is, to the extent of half of it, " beyond our reach in the present "—and, if we anticipate the use of the other half, it is, to the same extent, " beyond our reach " for ever. Moreover, it is " impossible to anticipate . . . [the] . . . use " of $Y$ except at a loss of half of it.

For all three concepts (the cumulable unit, the non-cumulable unit, and the quasi-cumulable unit which is either cumulable or non-cumulable as well) to fit Professor Hayek's statement about the effect of the irreversability of time, the statement would seem to require adjustment in some such manner as the following: " The underlying facts . . . are that of the fixity at some future point of time, or the non-anticipatability until some future point of time, of some of the services of certain resources, which puts them beyond our reach in the present and so makes it impossible to anticipate their use in the present, and the fact that other services of those resources can as a rule (?)[3] be made to occur in the present or in the future and thus can be postponed to the future, or, being anticipatable in the present only at a (great ?) loss of physical capacity of the resource to give rise to services, can be postponed to the future at a saving of the physical capacity which would be lost by anticipation ".

III. Let us now revert to the situation in which the units of service consist of only $X$, $Y$ and $Z$, none of them being quasi-cumulable. I have already suggested (in sub-section I of this section) that Professor Hayek would apparently regard such a resource as a permanent one. The resource provides

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[1] I am not discussing the question of what is the *usual* dispersion of the units.

[2] If it were not so postponed, the other half-unit would *not* remain to be decumulated later.

[3] I am not discussing the question of what is the usual dispersion of the units or the usual effect of quasi-cumulability on the distribution of capacity.

> " services which will continue permanently to be available independently of any deliberate action taken to provide them " (*op cit*., p. 50), and the existence of such a

resource would apparently burke Mr. Kaldor's denial referred to in the following statement:

> " If anyone wanted seriously to deny (as I understand Mr. Kaldor does) that there are such things as permanent resources, he would have to assert that it is conceivable that by raising the rate of consumption (or rate of output) to the highest level obtainable in the near future, the productive capacity of the more distant future could be reduced to zero." (*Op. cit*., p. 53, footnote.)

If, then, the criterion of permanence contained in these statements is adopted, the resource appears to be a permanent one. But not so a unit of service thought of separately. If we concentrate our attention upon one of the units, say *X*, " it is evident that [it] will have to be treated as non-permanent in the other sense because [its] services, once they become available, are non-recurrent "[1]: the unit *X* must be decumulated at *A* or not at all. If in the following quotation we read " units " for " assets " and " later units " for " permanent resources ", the statement would apparently apply to the units of the resource.

> " The non-permanent . . . assets cannot be *directly* used to contribute to the output of the time when they have ceased to exist. In so far as their existence does help to maintain output permanently above the level at which it could be kept with the help of the permanent resources alone, it must do so in an indirect manner." (*Op. cit*., p. 53.)

It applies to the unit *X*, because *X* " cannot be *directly* used to contribute to the output of " the point of time *B*, which succeeds the point of time *A* at which *X* occurs. " In so far as [its] existence does help to maintain output permanently above the level at which it could be kept with the help of [*Y*, occurring at *B*] alone, it must do so in an indirect manner ": it must be decumulated at *A* and invested.

With similar adaptation, the preliminary statement of the intended use of the term " capital " would apply to the units of the resource:

> " The term *capital* itself, in so far as it is required to describe a particular part of the productive resources, will accordingly be used here to designate the aggregate of those non-permanent resources which can be used only in this indirect manner to contribute to the *permanent* maintenance of the income at a particular level." (*Op. cit*., p. 54.)

If *X* were to be used to contribute to the permanent maintenance of the income, at *B* and *C*, at some level above that allowed by the decumulation of *Y* and *Z*, it could be so used only in an indirect manner—by investment. We appear to be in doubt as to whether to consider the decumulation and use of the *units* of this resource as the use of a permanent resource or as capital.

The difficulty of deciding whether a resource is permanent appears to be obviated if the criterion of permanency is derived from the view of the decision-maker, and if the idea of adequacy at all points of time, as well as the idea of mere lastingness, is introduced into the criterion. That the criterion should be derived from the view of the decision-maker is admitted by Professor Hayek when he writes:

> " What matters is not permanency in any absolute sense, but the opinion of the economic subject as to whether particular resources at his command will last throughout the period in which he is interested (be it his lifetime or a longer period), or whether they will be exhausted or used up earlier than this." (*Op. cit*., p. 52.)

But the criterion indicated in this quotation, even if it allowed us to ignore the question as to whether the resource were physically *capable* of being exhausted before the end of the period (i.e., whether its units occurring at the end of the period were cumulable or quasi-cumulable at earlier points of time), would not tell us anything about the *adequacy* of the resource to maximise returns at any point of time. If " the surplus units " occurred, the resource could be said to be adequate as well as lasting.

IV. " It has recently been suggested that . . . the reproducible or augmentable resources ought to be considered as representing ' capital '. But . . . the point that is relevant for our problem is not that certain existing resources *can* be replaced by others which are in some technological sense similar to them, but that they have to be replaced by something, whether similar or not, if the income stream is not to decline." (*Op. cit*., pp. 91 and 92.)

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[1] *v.* footnote 2 on p. 238.

I presume that, in the time-series representing the whole plan of decumulation of a production unit which was to use a non-permanent resource plus the something added, the additional units of service occurring owing to the addition to the resource would *have the appearance* of arising out of *augmentations* in so far as they were looked upon as increasing planned decumulation at a particular point of time, and of arising out of *replacements* in so far as they were looked upon as succeeding other units in time.

## III[1]

The concepts introduced in Section I may be used to clarify an issue raised by Mr. Hawtrey in his review of Professor Hayek's " Pure Theory of Capital " (*Economic Journal*, June–September, 1941). Mr. Hawtrey remarks:

> " The distinction between permanent and non-permanent resources is merely that the former, in the view of the economic subject, do not need a depreciation allowance, whether because they are so long-lived that the appropriate allowance would be negligible, or because their future usefulness cannot be sacrificed to an application in the present. Surely not a defensible line of demarcation! A depreciation allowance is required in all cases without exception."

And

> " The contribution of the first cost of any instrument to the cost of each unit of its product takes the form (apart from interest) of an apportioned part of depreciation . . . depreciation is inversely proportional to the life of the instrument . . ." (pp. 281 and 282).

Let us assume for simplicity that the budgetary period has been reduced to two points of time *A* and *B*, and that the decision-maker is working to maximise his net revenue. And, without in the first place making any definite assumptions as to the size, in relation to the expected possibilities of disposal after decumulation, of the number of units occurring at a particular point of time, let us suppose that all the units occurring at both *A* and *B* are non-cumulable units which are not quasi-cumulable.

It is surely almost self-evident that, in any case of this kind, if the maximum anticipated net revenue, calculated without making any " charge for depreciation " as a cost, expected to be achieved by the disposal of units occurring at *A* were, say, £1[2], and at B were, say, £1,000, no recalculation which, at either point of time, included a " charge for depreciation " as a cost could possibly, by limiting the anticipated disposal of units occurring at either point of time, increase the anticipated net revenue above £1,001. It is, of course, perfectly true that if, at the budgeting point of time, the decision-maker had not yet acquired the resource in question, he would not, on the given budgetary data, be likely to acquire it unless he were able to do so at a sacrifice which he considered to be less than the £1,001 that he expected to receive by disposing of the units of the resource's services occurring throughout the budgetary period. But, there being no cumulable units and no quasi-cumulable units, and consequently no opportunity of disposing of any particular unit alternative to the opportunity depending upon decumulation at the time of the unit's occurrence, his anticipated optimum disposal of units occurring at each point of time would, whether he had yet acquired the resource or not, be determined as that which would maximise the anticipated revenue (*net* revenue if there were, e.g., costs of disposal) achievable by his disposing of the non-cumulable units occurring at that point of time without " charging depreciation ".

Let us suppose, for example, that the decision-maker could acquire, or had acquired, the resource for £4; that the only units of service that arose from the resource were two non-cumulable units which were not quasi-cumulable, one at *A* and one at *B*; and that the decision-maker, considering, with Mr. Hawtrey, that " the contribution of the first cost of any instrument to the cost of each unit of its product takes the form . . . of an apportioned part of depreciation ", consequently refused to consider disposal of the unit occurring at *A* at anything less than £2. If the anticipated maximum price that anybody would pay for the unit occurring at *A* were £1, the decision-maker would refuse to consider disposal of the unit. If £1,000 were the anticipated maximum price that anybody would pay for the other unit occurring at *B*, the decision-maker's anticipated net revenue would (other possible costs ignored) be £1,000 –

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[1] There is probably nothing in this section which, in so far as it is sound, has not already been said or implied by economists working erosively upon accountism. See, e.g., twelve articles by R. H. Coase in Vol. XCIX of *The Accountant*.

[2] I use monetary terms for clarity and conciseness, knowing that what the decision-maker may regard as cost and revenue may include other elements than money payments and money receipts.

£4 = £996, instead of the £1,001 − £4 = £997 that he might have expected to make had he not insisted on " charging " the " apportioned part of depreciation " to the unit occurring at *A* that he might have planned to dispose of.

Now suppose we change the conditions somewhat and assume instead that, at the budgeting point of time, the decision-maker had already acquired the resource at a " first cost " of £3,000. By " charging " £1,500 to each unit, he would, if the anticipated demand conditions were as before, stand to lose £1,001 more than he need.

" First cost " is completely irrelevant when, at the budgeting point of time, it is already *ex post*. If it is still *ex ante*, it is relevant only as an indivisible cost of the resource (i.e., of the whole bulk of the units arising from the resource) to be set against the total anticipated net revenues, maximised without reference to the " first cost ", to accrue from the disposal of the units of service occurring over the budgetary period. If it is less than such total anticipated net revenue, the venture proceeds; if it is greater than the total net revenue, the venture is abandoned *ex ante*.

The complete absence of depreciation charge in these first three cases—it is *nil*, not infinitely, or indefinitely, small—does not depend upon a belief on the part of the decision-maker that the resource is " so long-lived that the appropriate allowance would be negligible "; it depends upon the fact that the decumulable units are non-cumulable, and not quasi-cumulable, and consequently have no alternative opportunity at another point of time. The crucial point is not that the resource is permanent in the sense of everlasting, but that the units that arise from it are absolutely specific (fixed) as to the time at which they are decumulable.

Where cumulable units occur during the budgetary period, however, something that may be regarded as a " depreciation charge " can occur, though it is still certainly not a " contribution of the first cost " of the instrument. Let us change one of our assumptions by assuming that all the units occurring during the period are cumulable units which are not quasi-cumulable. And let us suppose either that the decision-maker is contemplating the purchase of a resource whose " first cost " will be £1,000, or that he already owns the resource, its " first cost " having been £1,000, £1 Mn., or 1d., or anything else; that he expects it to provide him with five cumulable units, cumulable either at *A*, or at *B*, or between both *A* and *B* in any proportion; and that he expects the list of net revenues (gross revenues minus selling costs) that he could achieve by decumulating various numbers of units at *A*, and selling them, to be as follows:—

| *Sales of units decumulated at A.* | *Anticipated net revenues*, £'00. |
|---|---|
| 1 | 3 |
| 2 | 4 |
| 3 | 9 |
| 4 | 12 |
| 5 | 15 |

and the list of net revenues that he could achieve by decumulating various numbers of units at *B instead*, and selling them, to be as follows:—

| *Sales of units decumulated at B.* | *Anticipated net revenues*, £'00. |
|---|---|
| 1 | 5 |
| 2 | 7 |
| 3 | 8 |
| 4 | 11 |
| 5 | 13 |

We will assume that these markets exhaust the opportunities for disposal of the resource or its services.

It will easily be discovered by inspection that the plan which will maximise the decision-maker's anticipated net revenue, without taking the " first cost " of the resource, into account, will be a plan to cumulate and decumulate four units at *A* and one at *B*, and to sell them—for £1,700. The £1,700 shows the decision-maker a profit of £700 over the £1,000 which he contemplates paying for the resource, assuming that he has not already acquired it. If he has already acquired it, the £1,700 is none the less the most he can expect to make.

In behaving in this way, the decision-maker will, in effect, have planned to dose his cumulable units into the two markets in such a way as to maximise his total net revenue. The phenomenon that may be regarded as a " charge for depreciation " (as an opportunity cost) emerges if we argue as follows.

It is clear that, as the number of units is limited, by planning to decumulate a first unit at *A*, the decision-maker sacrifices the expectation of the *additional* net revenue to be achieved by decumulating at *B*, and selling five units instead of four.

This sacrifice we may, if we like, regard as a “ depreciation cost ” of planning to decumulate a first unit at *A*. By planning to decumulate a second unit at *A*, the decision-maker sacrifices the expectation of the *additional* net revenue to be achieved by decumulating at *B*, and selling, four units instead of three. This sacrifice we may regard as a “ depreciation cost ” of planning to decumulate a second unit at *A*. And so on. In the following table the “ depreciation costs ” have been worked out in this way, and deducted from the relevant additions to net revenue (before “ charging depreciation ”) expected to be achievable by increasing sales of units decumulated at *A*. The resulting additions to net revenue (*after* “ charging depreciation ”) have been cumulated to discover the maximum cumulative total increase, and hence the optimum decumulation at *A*. The optimum is four units.

| | “ Depreciation cost ” of increasing decumulation of units at A from 0 to 5 = sacrifice involved in reducing decumulation of units at B (and sales) from 5 to 0 | Increase in anticipated net revenue at A when decumulation is increased from 0 to 5 and the units are sold. | | Cumulative total increase in anticipated net revenue (after “ charging depreciation ”) as decumulation is increased at A and the units are sold. |
|---|---|---|---|---|
| | | Before “ charging depreciation ” | After “ charging depreciation ” | |
| From | £’00 | £’00 | £’00 | £’00 |
| 0 to 1 | 13 – 11 = 2 | 3 – 0 = 3 | +1 | +1 |
| 1 „ 2 | 11 – 8 = 3 | 4 – 3 = 1 | – 2 | – 1 |
| 2 „ 3 | 8 – 7 = 1 | 9 – 4 = 5 | +4 | +3 |
| 3 „ 4 | 7 – 5 = 2 | 12 – 9 = 3 | +1 | +4[1] |
| 4 „ 5 | 5 – 0 = 5 | 15 – 12 = 3 | – 2 | +2 |

[1] Optimum

If in the argument we had written *A* instead of *B*, and *B* instead of *A*, we should have obtained the following results, indicating the optimum decumulation at *B*. The optimum is one unit.

| | “ Depreciation cost ” of increasing decumulation of units at B from 0 to 5 = sacrifice involved in reducing decumulation of units at A (and sales) from 5 to 0 | Increase in anticipated net revenue at B when decumulation is increased from 0 to 5 and the units are sold. | | Cumulative total increase in anticipated net revenue (after “ charging depreciation ”) as decumulation is increased at B and the units are sold. |
|---|---|---|---|---|
| | | Before “ charging depreciation ” | After “ charging depreciation ” | |
| From | £’00 | £’00 | £’00 | £’00 |
| 0 to 1 | 15 – 12 = 3 | 5 – 0 = 5 | +2 | +2[2] |
| 1 „ 2 | 12 – 9 = 3 | 7 – 5 = 2 | – 1 | +1 |
| 2 „ 3 | 9 – 4 = 5 | 8 – 7 = 1 | – 4 | – 3 |
| 3 „ 4 | 4 – 3 = 1 | 11 – 8 = 3 | +2 | – 1 |
| 4 „ 5 | 3 – 0 = 3 | 13 – 11 = 2 | – 1 | – 2 |

[2] Optimum

To plan for these two interdependent optima will be to plan for the following revenues:—

| | £ | £ |
|---|---|---|
| *Sales of units decumulated at A.* | | |
| Net revenue after deducting “ depreciation ” | 400 | |
| “ Depreciation charged ” (£200 + £300 + £100 + £200) | 800 | |
| | | 1,200 |
| *Sales of units decumulated at B.* | | |
| Net revenue after deducting “ depreciation ” | 200 | |
| “ Depreciation charged ” | 300 | |
| | | 500 |
| Total revenue | | £1,700 |

The total “ charge for depreciation ” (£1,100) has no relationship to the “ first cost ” of the resource. Its components, £800 and £300, are simply sacrificed alternative anticipated revenues.

At least on the assumption that the decision-maker is allowed to, and does restrict the sale of his units in order to maximise net revenue, even this type of "depreciation cost" disappears if the number of cumulable units occurring is great enough in relation to the expected possibilities of their sale after decumulation. Let us suppose that, if the above lists of anticipated net revenues were continued, the anticipated net revenues for sales of more than five units decumulated at *A*, or of more than five decumulated at *B*, would begin, and continue, to fall. This would mean that the decision-maker would consider that no additional net revenue could be earned by selling more than five units decumulated at *A*, or more than five decumulated at *B*. And let us suppose also that six instead of five cumulable units occurred. Now it is obvious that no "depreciation charge" would be made, either at *A* or at *B*, in respect of the first unit contemplated to be cumulated and decumulated there, for the simple reason that so to cumulate and decumulate the unit at *A* (or *B*) would still leave enough units (five) to maximise anticipated net revenue at *B* (or *A*) thought of separately. But the second unit would be "charged" with "depreciation" to the extent of the *additional* net revenue expected to accrue from selling five instead of four units at the other date . . . and so on. If seven units occurred instead of six, the first two units would be free of "depreciation charge" . . . and so on. If ten units occurred, the "depreciation charge" would disappear altogether, because anticipated net revenue could be maximised in respect of sales of units decumulated at both *A* and *B*, separately: the decumulation of five at *A* (or *B*) would not prevent the decumulation of five at *B* (or *A*).

It is at this point that our resource becomes a permanent[1] one according to the definition given in Section A: "the surplus units" begin to appear. The decision-maker has "just enough" if he has ten units; "too many" if he has eleven. No "charge for depreciation", even of the opportunity cost type, is required when the resource is a permanent one in this sense.

The argument has abstracted from quasi-cumulability. The introduction of quasi-cumulability would, however, require no change in principle in the appropriate way of regarding "depreciation". If a particular unit were expected to be, say, half-decumulable at one point of time, the appropriate "depreciation charge" for the planned decumulation of the half-unit at that point of time would apparently be the sacrifice of the expectation of the additional net revenue to be achieved by decumulating an extra full unit (or possibly part of a unit, if the unit were quasi-cumulable at other points of time) at the point of time at which such extra decumulation was expected to be possible and was expected to add most to total net revenue by its sale.

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[1] This is not strictly true unless the selling costs referred to above are deducted by another production unit before the revenue is paid to this one. If they are costs planned *ex ante* by the decision-maker to be incurred by this production unit in the course of the budgetary period, the resource becomes a non- or quasi-permanent one.

# " Elastic Expectations " and the Austrian Theory of the Trade Cycle

By L. von Mises

In the thirty-one years which have passed since the first edition of my *Theory of Money and Credit* was published no tenable argument has been raised against the validity of what is commonly called the " Austrian " theory of the credit cycle. It was easy to prove that all objections brought forward were either futile or founded on a mistaken interpretation of the doctrine attacked.

However, some remarks made by Dr. L. M. Lachmann in his recent article " The Rôle of Expectations in Economics as a Social Science "[1] deserve careful consideration. In this thoughtful essay the author contends that " the Wicksellian theory appears to be based on a very special assumption, viz., of a capital market without a very strong mind of its own, always ready to follow a lead on the spur of the moment, and easily led into mistaking an ephemeral phenomenon for a symptom of a change in the economic structure. Without fairly elastic expectations there can be no crisis of the Austro-Wicksellian type " (cf. p. 23). I fully agree with this statement with exception of the use of the term " elastic ", which I would rather avoid as it implies an inadequate and misleading mechanical metaphor; furthermore, I believe that the word " boom " should be substituted for the word " crisis ".

But I want to point out that I did not fail to state the fact that my explanation of the trade cycle is based on such an assumption. In my treatise *Nationaloekonomie* (Geneva, 1940) the chapter dealing with credit expansion ends with the following observation: " This typical course of the trade cycle has sometimes been altered through the interference of extraordinary events, mostly of a political character. But by and large one can observe in Great Britain since the end of the 18th century and in Western and Central Europe and in North America since the middle of the 19th century an almost regular alternation of booms and depressions. We may wonder whether the conditions which have had this result still prevail. The teachings of the monetary theory of the trade cycle are to-day so well known even outside of the circle of economists, that the naïve optimism which inspired the entrepreneurs in the boom periods has given way to a greater scepticism. It may be that business men will in future react to credit expansion in another manner than they did in the past. It may be that they will avoid using for an expansion of their operations the easy money available, because they will keep in mind the inevitable end of the boom. Some signs forebode such a change. But it is too early to make a positive statement " (pp. 696 and 697).

It is necessary to realise that market conditions brought about by credit expansion are such that the individual businessman cannot find any fault with the situation if he is a businessman only and does not view things with the eye of an economist. There is, on the one hand, an increased demand for goods, prices are rising, and on the other hand there is an ample supply of credit at comparatively low rates of interest. There are further would-be experts who repeat again and again that good business is the outcome of some material factors, for instance technical improvements, and has nothing to do with purely monetary and credit phenomena; there is no reason, they say, to fear a change as mankind has finally entered the stage of everlasting prosperity.

Even a businessman who looks askance at the low level of interest rates brought about by credit expansion finds it hard to realise that the country or the world has really embarked upon easy money policy. For interest rates are—with the exception of the very beginning of the credit expansion—mostly not low when compared with the level which business used to consider as normal. They are low when measured by the standard which they would have to reach in a period of progressive inflation and its corollary, a general rise of prices, since they would have to include at such a time a compensation for the depreciation of the money unit going on in the period of the loan. But in order to understand this, the entrepreneur would have to be familiar with the relation between depreciation and the gross rate of interest.

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[1] Economica, X (n.s.), No. 37 (Feb. 1943), pp. 12–23.

The economic consequences of credit expansion are due to the fact that it distorts one of the items of the speculator's and investor's calculation, namely, interest rates. He who does not see through this, falls victim to an illusion; his plans turn out wrong because they were based on falsified data. Nothing but a perfect familiarity with economic theory and a careful scrutiny of current monetary and credit phenomena can save a man from being deceived and lured into malinvestments.

It is worth mentioning that the factors described by Dr. Lachmann provide one of the two arguments proper for a thoroughgoing refutation of the spurious acceleration theory of the trade cycle. This theory considers the entrepreneur as an automaton mechanically reacting to every increase of demand by an expansion of investment; Dr. Lachmann's reasoning must logically lead to an exposure of these fallacies. It is, of course, a minor objection only to be raised against the acceleration doctrine; the main argument is that but for credit expansion such a tendency toward additional investment would very soon be checked by the rising prices for the factors of production concerned. (Cf. my *Nationaloekonomie*, pp. 530–532.)

It is not the purpose of these cursory remarks to appraise the full content of Mr. Lachmann's contribution. I had nothing else in mind than to recall the fact that the "Austrians" are not guilty of having neglected an essential assumption of their theory.

I wish, however, to stress one more point. Why call this monetary or credit expansion theory of the trade cycle the "Austrian" or the "Austro-Wicksellian" theory? Of course, I am very grateful for the honour paid in this way to me and to my country. But why forget that this theory is a continuation, perfection and generalisation of the Currency theory? Neither Wicksell nor I myself nor Professor Hayek have ever forgotten to emphasise this point.[1]

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[1] On pp. 518–519 of my *Nationaloekonomie* I have tried to demonstrate in what regards the teachings of the Currency School were defective and had to be amended and supplemented by a more general theory.

# John Rae and John Stuart Mill: A Correspondence

[When about forty years ago Professor C. W. Mixter, the rediscoverer of John Rae's *New Principles of Political Economy* (1834), was preparing the new edition of this work, and collecting for it what information about Rae's life could be gathered, he gave in two articles and in his introduction to the new edition several extracts of drafts of letters by Rae to Mill, written about 1863 but perhaps never sent, and mentioned a letter written by Mill to Rae, apparently about the same time, but never received by Rae. (*Quarterly Journal of Economics*, Vol. XVI, Nov., 1901, pp. 123–125, *Economic Journal*, Vol. XII, 1902, pp. 111–120 and *The Sociological Theory of Capital*, 1905, Introduction.)

Among the papers of the " Mill-Taylor Collection " in the Library of the London School of Economics the following letter by Rae to Mill, written nearly ten years earlier, has been found, together with the draft of Mill's reply. The latter will, it is hoped, ultimately be included in the new edition of Mill's letters which is being prepared. As it may be some time, however, till this edition reaches the date to which the present letters belong, and as the draft of Mill's reply (which was almost certainly sent but perhaps never reached Rae) contains a point of some interest in the history of Economics, it is here reprinted together with Rae's letter.—*Ed.*]

### 1. John Rae to John Stuart Mill

Koali, Hana, Maui, Hawaiian
or Sandwich Islands, Dec. 5, 1853.

Sir,

I am induced to write you from having some months since perused an extract from your work on Political Economy, in which you notice a previous work of mine on the same subject. A friend in Canada sent me the extract, and you will believe it afforded me considerable gratification. Indeed, I may say, it is the only thing connected with that publication, which has afforded me any gratification. I had in truth given up the hopes of my work making any impression whatever, unless I should happen to attract public attention by other efforts, in some other sphere of literary enterprise. In that case I meditated, and still meditate, writing a treatise on Banking and Currency, the true principles regulating which I have long conceived can be clearly deduced from what I may venture to call *my* doctrines, and scarcely otherwise. I regret that I have had no opportunity of perusing your work, and so know not how far our ideas coincide, or diverge, on this and other matters.

I have been a wanderer in California, lost my health there, came to these islands to recruit about three years since, have been practising my profession, I am a medical man, and am now engaged in rather extensive agricultural operations, and some other material projects, which will I expect, for some years, very much occupy my time. I have, however, lately written an article, unfolding some views in Geology, which are, I believe, novel, and even to myself somewhat startling, but yet, I think, will ultimately be found true. Certain phenomena in these islands, and on this ocean, led me to them. This will probably appear in *Stillman's Journal*, though there may be some delay from an apprehended miscarriage of part of the manuscript. I have also written, for a medical friend in Canada, some observations on medical matters, which, it is likely, will find their way to the American press. I have, too, made some progress in a work on the condition and prospects of these islands, which I should like to publish simultaneously in England and America. I have besides an essay on hand on the Hawaiian language. It is this latter labour that is the occasion of my presently addressing you.

In my endeavours to get as nearly as possible to the root of the matter, I have found my way to, or I have stumbled on things that seem to me new and strange or, rather, which will appear such to others, for to me they only seem confirmatory of certain notions I have long entertained concerning the real history of Man, which you will probably put it down as a grave heresy in me to say, seems not to me to have been hitherto treated of, in the true and comprehensive spirit of Baconian philosophy. At all events the subject as it presents itself to me, besides being really curious, seems to open up glimpses at least, of paths stretching far away into the recesses of long antecedent time, which well deserve to be explored. I am somewhat nervous, and you will allow I have a little reason to be so, about bringing this forth. I conceive it is not likely to be well judged, or at all indeed appreciated, but by men of somewhat large and reflective minds. Fancy not that this is vanity. It is my knowledge of its probable imperfections that makes me feel the need of such judges. Were I writing in a well stored library, and with ample leisure, I should not be at all uneasy, but I do not relish that, for want of full development, or a slip here and there, views, which, though novel, have, I conceive, truth and philosophy for their basis, should be thrown aside as merely fanciful or phantastical, or perhaps absurd and ridiculous. I am desirous therefore of audience fit, though few, and this has led me to take the liberty of addressing you. It is perhaps, therefore, proper, at all events it seems to me not unmeet, for me to tell you what I really have been and am about.

I have long entertained a project, partly literary and partly practical, which, as I advance, wears more and more a definite shape, to which all I have hitherto done or attempted, or that I have indicated to you, are mere toys. It must in fact either fail altogether, or must give a new impress to humanity itself. As a means to this end I desire a certain literary and philosophical reputation, and a certain amount of capital. I went to California hoping there to find the latter, and the means to the former. To gather gold and matter for a book. I imagined that a series of geological notes, a science of which I have long been a votary, and some account of the scenery and scenes the country exhibits, might make a volume which would have a certain circulation. But my health gave out, and I nearly died. On recovering, in these islands, I thought that it was but changing the scene of my operations, and that they might serve me to make money and write a book. This is what I am attempting. It was far from my wishes to go very deep into things, for that is voted heavy, I merely desired a light volume, somewhat tinged with the spirit of philosophy, that might have a certain popularity. But the bent of my mind, or some fatality has ever opened up to me large though sometimes misty views of things others have passed by, in every subject I have studied, and, against my will it has been so here. Hence my separate Geological and philosophical essays. There is this comfort, both will be short.

Koali, Jan. 9th, 1854.

I have been detained in this quarter of the island much longer than I anticipated and as I have to deliver this letter to the post office Lahaina myself the delay has given me leisure for some farther reflections on the mode of appearance in type best suited for my purpose. The result of these is embodied in the following request. Would you do me the favour of superintending, or rather get someone to superintend, the printing in London of a pamphlet or small volume of 80 or 100 pages, a sort of epistle to the *eruditi*, the *viri graves et docti* of the European republic of letters. The issue not to exceed 200 and to be considered strictly private. I would remit the funds necessary to defray the expense. In this production I could open myself more freely than in any publication, and I should hope from some quarters to receive advice and criticism that might be of much service to me in the specific philosophical problem and on others connected with it and might prepare the way for a real publication. It would be necessary that I should be edited and thus receive the approbation of some one in its own department of science for his name to secure attention. Have you any friend in the literary circles of London or of Paris or in Germany who could do this, and if so, would you be willing to ask him to do it ?

I have been so long out of Britain that absence or death has taken from me all literary or other acquaintances within that Kingdom. This will, I hope, be handed you by my particular friend Hugh B. Willson, Esq., of Canada west, whom, if you think well of it, you can inform as to the expense of paper, printing, etc. I write shortly on this subject to Sir Francis Bond Head, who may perhaps have an interest in my doings from some passages in our Canadian Rebellion. To Dr. Neil Arnott, who was once tutor to my elder brothers, but whom I fear death or indisposition may have withdrawn from the world, and to Dr. William Beattie, who is, I think, a native of the same town with myself, Aberdeen, and whom if so I knew in Edinburgh.

I shall hope to hear from you as soon as may be and you will oblige me by informing me how my work happened to fall into your hands. Very few copies went to G. Britain. Mr. W. will forward any letter from you.

With much respect

I am,

your very obedient servant

John Rae.

John Stuart Mill, Esq.,
England.

## 2. John Stuart Mill to John Rae (draft)

Sir,

Your letter of Jan. 9th has reached me within these few days. I am glad to hear of the various literary enterprises you have in hand or in contemplation, as I feel assured from the character of your work on Pol. Ec. that your speculations in any subject to which you have applied yourself will contain (whether I agree with them or not) enough both of knowledge and of originality and ingenuity to more than justify bringing them before the world. I have made more use of your treatise than you appear to have been informed of, having quoted largely from it, especially from your discussion of the circumstances which influence the " effective desire of accumulation ", a point which you appear to me to have treated better than it had ever been treated before. I have already published my opinion that nothing was wanting to your book except favourable chances to have gained you the reputation you desire, and which I hope you may acquire by other writings.

You could not, however, have addressed yourself to any person less capable than myself of giving any useful assistance in bringing out your speculations on the Hawaiian language. My own pursuits do not lie in the direction of comparative philology nor have I any acquaintances in this class of érudits (chiefly to be found in Germany) from someone of whom you desire a recommendation and his name as editor. Nor do I think this would easily be obtained for the preliminary pamphlet which you contemplate whatever might be the case with the completed work. Even to get the pamphlet printed is more than I am able to undertake, not only from pressing occupations, but because the state of my health renders my residence in England, at the time when your MS could reach me, extremely uncertain.

Dr. Arnott, whom you mention as an old acquaintance, is alive and flourishing and may possibly have it more in his power to promote your object than myself.

You ask me how your book became known to me. I first heard of it from Mr. Senior who recommended it to me as a book of which he had a high opinion, and after I had read it through his means I picked up a copy in a stall.

I hope your health is quite re-established.

I am, Sir,
very faithfully yours,

E[ast] I[ndia] H[ouse], London, Sept. 19th, 1854.

# Book Reviews

*Economic Analysis.* By KENNETH E. BOULDING. Harper & Brothers, Publishers, New York and London. 1941. xviii + 809 pp.

When this book, published in the United States two years ago, becomes available in this country in adequate numbers this will be an event of some importance for the teaching of economics. Nothing has made the teaching and study of economic theory so difficult for many years as the lack of a satisfactory text-book in which the more important modern developments were fully incorporated. We have recently been given one or two excellent first introductions, but the student on a somewhat more advanced level had to be sent to a large number of works and articles with little apart from the lecture courses to connect the bits into a coherent whole. As a result lecturers, instead of being able to spend most of their time on explaining and illustrating the main theorems, had to aim in the first instance at covering the whole field without missing a single step; and the student could hardly be expected to fill for himself any gaps from the literature.

This state of affairs was, perhaps, inevitable as a result of the rapid progress of economic theory in the past fifteen years. The task of selecting the essential from the refinements of secondary importance, and of embodying the important new tools of analysis, not merely into a coherent system, but also into a readable and not unduly difficult exposition, is one which called for unusual judgment and skill. If after this I say no more than that the present book is by far the closest approach to the needed advanced text-book that I have yet seen, I hope this will be understood as the high praise it is meant to be. Whether the ground it covers will ultimately prove the lasting core of the teaching in economic theory or whether some sifting out may not be necessary, experience will have to show. But there can be no doubt that the substance of the book covers essentially the ground which in recent years has become the content of the courses in economics in the main centres of the teaching of economics.

Professor Boulding has brought to his task the valuable combination of an English training with more recent experience as a teacher in the United States. The book is, as the title indicates, strictly confined to theory and any criticism on the ground that it is too purely theoretical would therefore be beside the point. This is not to say that every effort should not be made in such a book to illustrate the relevance of the theoretical proposition by concrete illustrations. And the author is on the whole very successful in the choice of examples introduced for this purpose. If the book remains nevertheless on the whole severely theoretical and makes little attempt to combine the descriptive or applied sides of economics with the exposition of theory, this is in the opinion of the reviewer both necessary and desirable. Economic theory can be properly understood only as a whole and any attempt to intersperse the teaching of theory with too much descriptive matter is likely to form an obstacle rather than a help to an appreciation of its significance. But no teacher ought of course to fail to warn his students that by mastering the theory of his subject he has made only a first step in the direction of becoming a competent economist.

The book is rather traditional in the prime emphasis it puts on the "real" as distinguished from the monetary aspects of economic theory. In a few years' time we shall probably all wish that it had gone further in the attempt to integrate general and monetary theory. But it is perhaps premature to expect a textbook to succeed where opinion is still so much in a state of flux and on the whole it is probably better that at a time when the interest of most teachers is on the monetary aspects the basis text-book should rather over-emphasise the "real" side.

The exposition is divided into two parts of which the author says in the Preface that in Part I he has "endeavoured to range over the whole field of economic analysis, using the simplest possible weapons of analysis so that the student may obtain a rough outline of the whole picture. In this part, therefore, I treat the concepts of demand and supply curves as self-evident, without discussing the marginal analysis which underlies them. With the concepts of demand and supply as principal instruments of analysis, I outline the main principles of price determination and of distribution. In this part I also consider the theory of money, banking, and international trade, still confining myself to the theory of demand and supply. In the second part I give in detail the marginal analysis underlying the demand and supply curves, including the theory of the individual firm, of consumption, of imperfect competition and monopoly, together with a discussion of the theory of capital."

Though on this plan the first part of the book covers essentially the ground of a first year course, I should recommend it less as a first year book than for purposes of revision for the student starting on his second year. The part of the book which is so valuable because no other treatment of economic theory on a comparable level is available, is the second part. Here the student gets a clear and sufficient though not over-elaborate introduction into the modern theory of consumption, production and competition, and the essential diagrammatic apparatus evolved in this connection. Though it is difficult to be certain, in view of the volume of the recent literature, it seems that the author has not confined himself to take over the work of others, but has introduced several original and useful constructions of his own—even outside the theory of capital where the exposition is based largely on his own earlier work. But the diagrams which are new, at least to the reviewer, though some of them are at first startling by their unfamiliarity, prove on examination all to be useful condensation of important results of modern theory, and successfully designed with an essentially didactic purpose.

This brings me, however, to what appears to me one really serious defect of the book, no less serious for the fact that it is merely the result of the author conforming to established American text-book practice: the student is left practically without any guide to the literature in which the technique and the theorems he is taught have been elaborated, or the history of economic theory in general. Hardly a name or a title of a book is mentioned and the reader is evidently expected to confine himself to the study of this one book—or perhaps to it and a few other text books of similar nature on related topics. I regard this as thoroughly vicious and incompatible with the critical attitude and the sense of the living and progressive character of the discipline which seems to me essential to the study on a University level. If in the opening passage of this review I have deplored the necessity that in recent years no adequate systematic statement of the body of modern economic theory was available and that the student was forced to collect most of his knowledge from the original, and in some instances far too technical works, I should regard it as even more fatal if a single book should become the beginning and the end of his study and he should remain entirely unfamiliar with the original works. And the present book, with its omission of all references, rather encourages such an attitude. If it is to become as satisfactory as help to teaching in this country as it might be I am convinced that a separate English edition ought to be prepared in which this defect is remedied. The book need thereby not become larger nor even the typography of the text disturbed. If the questions added at the end of each chapter according to the American fashion were cut out and the space thus gained used for a brief discussion of the history and literature of the doctrines discussed, the value of the work would be greatly enhanced. I have no doubt that the author himself is well qualified to remedy this defect and thereby to increase the debt which teachers of economics owe to him.

F. A. v. Hayek.

*Economic Efficiency and Social Welfare.* By A. L. Macfie. Oxford University Press. 1943. xi + 142 pp. 10s. 6d.

This work is a contribution to the ethical side of economics, a side which economists are too apt to neglect, or indeed to disavow altogether. Dr. Macfie's theme is the social character of right conduct. " While modern science and ethics have accepted the revolution from egoism to sociality, there has been no such revolution in economic theory. The positive individualist approach is still in the citadel, though it is beleaguered and confined " (p. 22).

Further, " what we mean by an organism is a unity which is an *end to itself* as well as to each of its parts. This . . . defines in clear terms what we mean when we say, as Darwin said, ' man is a social animal ' " (p. 24).

The implication is that there must be a choice between an individualism or egoism, which assumes every man to seek his own good to the exclusion of the good of his neighbour, and a sociality which distinguishes the good of the community from the good of the individuals composing it. A dangerous view! Once an idea of the good of the community distinct from the good of its members is accepted, the door is open to sacrificing the good of the members to the only kind of good of the whole which gains recognition, that is to say, national power. That is far from Dr. Macfie's mind, and the sequel shows that by sociality he does not mean any such thing. He means no more than that every member of the community ought to aim at the good of the rest as individuals, " the defence, health, education and ability to enjoy work and leisure of the masses of the people " (p. 32).

He contrasts this with the egoistic hedonism which he imputes to the classical economists. And no doubt instances of economists who assumed that standpoint can be cited. But the utilitarian ideal of the greatest happiness of the greatest number was not egoistic. It was social in Dr. Macfie's sense, even if it was narrow in identifying good with happiness. Dr. Macfie rightly criticises the classical assumption that utility is quantitatively measurable, and supports the view that economic transactions only indicate preferences without affording a standard of measurement. (Chapter V.) He points out that the law of diminishing utility or diminishing marginal substitution is a static principle limited to a given system of wants (p. 76). And he describes the tendency towards satiety as a ' brake on the wheels ", to be overcome " by so varying and blending experiences that, instead of living in a world in which happiness is continually running down, we actually regard such a situation, though it is often actual, as a failure " (p. 77).

" No such system of equilibrium," he continues, " could have been developed on the basis of a view of effective desire which realised that at any point a recreation of ' tastes ', a re-shuffling of the ingredients of satisfaction, might cause a positive increase in the satisfaction derived from successive units of productive resources."

Surely this is a misreading of the principle of diminishing utility. Just because it is a static principle, the successive units of products are not successive in time, they are hypothetical alternatives to the actual number of units acquired in a given interval of time. Such alternatives viewed in anticipation are the basis of choice in practice.

Economists who employ the law in their reasoning do not exclude a principle of increasing utility, but the amount acquired of any product to which that principle applies is pushed to the limit at which either it changes to diminishing utility or the increasing utility is outweighed by the loss of satisfaction from the rival products displaced.

The case of increasing utility in a given system of wants is not of great importance. On the other hand it is a mistake to suppose that new satisfactions and acquired tastes do not find a place in classical economics. Nor does the recognition of a change in the underlying tendencies constitute a transition to a dynamic system. Such a change merely indicates a different equilibrium position. The characteristic of a dynamic system is that it takes account of the rates of change or at least of the relative rates of change involved in a state of transition.

In a penultimate chapter Dr. Macfie poses the question : is efficiency among the moral values ? Is it on the same plane as courage or temperance or integrity ? Efficiency he identifies with " economy ", which " in its negative sense is the process of avoiding waste. More positively it is the process of making the most out of scarce resources ". Here he is linking his subject with his earlier work, *An Essay on Economy and Value*, in which he followed out the implications of Professor Robbins's definition of economics in terms of scarce means.

In reality this has little relevance to his principal theme. His definition of efficiency is too wide. Indeed his previous book might be taken as something like a *reductio ad absurdum* of the scarcity definition of economics, when he extends it to cover the " economy " of voice production. An analogy between the equilibrium of marginal yield or of marginal utility and the maintenance of due proportion in the factors directed to any human accomplishment, artistic, scientific or moral, does not justify the inclusion of the latter in the term " economy ", so long at any rate as economy is linked with economics. Nor is this maintenance of due proportion the whole of " efficiency " properly understood.

Efficiency, whether in this limited sense or in its more comprehensive sense, is primarily a means, not an end. When it is enjoyed or admired, these states of feeling may be regarded as good in themselves, but that is true of any human action which is commended as beneficial. The question whether efficiency is a *virtue* is different ; it is a virtue and its lack a fault in so far as it is within the control of the will.

Dr. Macfie, in exploring the borderland between two great sciences, has inevitably encountered difficulties, but he has made his book really interesting and has presented new aspects of his subject which afford food for thought.

R. G. Hawtrey.

*A History of French Commercial Policies.* By F. A. Haight. New York : Macmillan. 1941. xviii + 286 pp. $5.50.

The tardy review of this book has been due to the war. It is a discerning study of international commercial relations during the inter-war period. The first hundred pages are devoted to the earlier historical setting—French tariff policy from Colbert to 1918. This is followed by an account of the trade problems of the last war and the repercussions upon post-war policy. The attempt to return to something like the relatively liberal pre-1914 system is then described ; and finally, the reaction during the period of economic and political crisis, from 1931 to the outbreak of the present war, is dealt with. Part IV of the book is really a supplement devoted to an account of French colonial commercial policy.

The immediate post-war problems to be faced in 1919 arose from employment questions and monetary and price complications. It was difficult to remove war prohibitions, although influential opinion in France was, on the whole, hoping for this. Various expedients such as the tariff co-efficients, had to be adopted as a means of countervailing inflationary price changes. The needs of the moment militated against any early return to the pre-war regime. Then, in the meantime, French liberal opinion was apparently pushed into the background through the force of world events. " Disillusioned of all prospects of political and industrial security based on international co-operation, France retained and intensified her protectionist regime " (p. 104) ; and so she seems to have been driven, with some reluctance, towards self-sufficiency.

According to Mr. Haight, it was the general absence in the world of " statesmen with sufficient courage, or power, to make a bold bid for economic recovery and political appeasement by removing the barriers to international commerce " which was to blame (p. 105). Whilst at the international conferences of 1920, 1923 and 1927 " lip service " was " paid to the desirability for more liberal policies ", the resolutions " received scant application in the legislation of individual nations " (p. 105). And in all countries of the world, statesmen used the " plea of special difficulties and ' new ' circumstances " to justify the pushing up of tariffs (p. 106).

France herself attempted at first to follow the incompatible policies of restricting German imports and pressing for reparations. Her dissatisfaction with the unconditional most-favoured-nation principle handicapped her negotiations during the first post-war years. She succeeded for a time in concluding new conventions on actual most-favoured-nation conditions but with the formal appearance of reciprocity. The restoration of German tariff autonomy, however, forced the issues to be realistically faced, and the Franco-German convention of 1927 was based upon unconditional most-favoured-nation co-operation of the traditional kind. Mr. Haight describes it as ' the greatest single contribution to liberal trading policies and to the cause of international co-operation in the twenty-five years since 1914" (p. 138).

The setback to the hopes aroused came owing to the " world economic crisis ", foreshadowed by events of 1929, and tumbling upon the nations in 1931. From that date, " the instruments of commercial policy changed almost beyond recognition " (p. 141). The virtual disappearance of capitalism in some countries and the increase of State control in others caused tariffs to become secondary restraints. Quotas and bi-lateral arrangements became the normal machinery of exclusion. Restraints were answered by counter-restraints and there emerged a period of " rampant economic nationalism in which . . . little attention was paid to legal niceties and treaty obligations. Rather than embark upon a fruitless controversy about the infringements of most-favoured nation rights, foreign governments adopted similar measures as a means of either protection or retaliation " (p. 163)

The facts of this depressing age in world affairs have been recounted by many others and the period can now be seen in fair perspective by most students of international trade. But in the book under review the facts are presented as seen by an observer in France.

After the devaluation of 1936, various gestures towards quota mitigation were made by the French. But there was no real response abroad, and the authorities were never willing to pursue a really effective policy of uni-lateral opening of markets. Thus it was that " the summer of 1937 marked the end of the abortive attempt at liberalism " (p. 185). The quotas were restored and duties raised. Then, for a couple of years, monetary questions overshadowed tariff and quota issues, whilst distrust and uncertainty became more and more obvious in international relations. " A world nominally at peace was commercially at war " (p. 188). There was a complete repudiation of the 19th century liberal experiment whch aimed at political security through increased friendly commercial contracts " (p. 192).

In a most enlightening chapter Mr. Haight endeavours to show how closely the changing form of industrial organisation in France (paralleled in a greater or less degree in all countries of the world) contributed to this result. He finds the key in the expanding field of State functioning and the growing concentration of ownership and private control " with the tacit consent of the State " (p. 199). " Without stretching the analogy too far," he says, " these tendencies may be compared with conditions under the *ancien régime*, when internal trade was regulated by guilds and foreign commerce was monopolised by companies enjoying official status " (p. 198). " Industry achieved a degree of co-operation which would have been regarded in the 19th century. as a serious menace to the authority of the government " (p. 199). The combines inevitably acquired a " preponderance of power " (p. 200) enabling them virtually to " dictate customs policies and make or break negotiations for commercial treaties " ; and this became " not the least of the purposes of syndicates " (p. 201). Opinion, and hence policies, were moulded by these interests through the press, parliament, the government and the administration.

At one time, some hopes of unshackling world trade arose through the development of international cartels. A measure of international economic rapprochement seemed not unlikely, and there were some apparent mitigations of the conspicuous trade barriers for such reasons. " But the more common experience was that the protective tariffs remained unaltered, if only because the cartels were never universal." Moreover, as the author asks, if the markets are divided by agreement, "does it matter very much whether tariffs . . . are retained or abolished ? " (p. 203).

Under the contemporary traditions determining the function of the State, Mr. Haight finds it " difficult to conceive . . . a policy of self-sufficient nationalism " not resulting (p. 208). " When the State descends to earth and gives of its spirit and purse to the organisations of mankind, it cannot afford to suffer loss or failure in any of its members ; for it must remain omnipotent and beyond failure lest it lose respect and prestige in the eyes of the citizens " (p. 208). The author concludes that the future " rests largely with the outcome of this struggle between irreconcilable theories of the proper relationship between State and individual " (p. 209).

Mr. Haight's book is to be welcomed firstly as a contribution to world economic history and secondly for its penetrating and challenging comment on the decadence of international relations.

W. H. HUTT.

*The Population of Bristol.* By H. A. SHANNON and E. GREBENIK. National Institute of Economic and Social Research. Occasional Papers II. Cambridge: at the University Press. 1943. 94 pp. and map. 7s. 6d.

This publication, while it is mainly of local interest, may well afford a pattern for the numerical history of the growth of a great city on a geographical or topical and analytical basis. Mr. Shannon's contribution, Chapter II, which forms one of the two parts of the study, relates to Migration, both within the enlarged city boundaries and beyond them. The population of the centre is stationary or diminishing, while circle or fringe after circle of the surrounding country has been urbanised. If one stands on a clear day on the highest point of the University one can see the process of expansion from the early city with innumerable churches to the 19th century buildings, villas or congested streets in the region shown on the map as the additions 1836–1895, and beyond some three miles away the red roofs of the most recent housing estates. In his rather scanty historical survey Mr. Shannon does not explain that the long arm extending down the right bank of the Avon to the Severn was the culmination of long competition between the ancient docks and basins dating from before the discovery of America, separated from the Severn by several miles of tortuous river, navigable only for a short period at each high tide, and the docks at Avonmouth, dating from 1880 or earlier, which were suitable for the ocean liners of that date. The ancient docks became the property of the City in 1848, those at Avonmouth were acquired in 1884, and in 1904 the city boundary was extended to include not only the satellite town and harbour five miles from the centre, but also the narrow strip of purely rural country on the right bank of the river, since then developed by garden suburbs.

One purpose of the study is to examine how far the population of the expanded urban area could have been supplied by the natural increase of the earlier residents in Bristol, and how far it must have depended on immigration, and further what regions had supplied the immigrants. The statistics only show *net* immigration, that is the excess of immigrants over emigrants; it is not to be supposed that all persons born in Bristol remained there.[1] Mr. Shannon does not use the information given in the 1911 Census. Thence we may find

ENUMERATED IN BRISTOL

| | |
|---|---|
| Born in Bristol .. .. .. .. .. | 233,000 |
| Born in one of six neighbouring Counties .. .. | 72,000 |
| Born in rest of U.K. .. .. .. .. | 48,000 |
| Born abroad .. .. .. .. .. .. | 4,000 |
| Total .. .. .. .. .. .. | 357,000 |

BORN IN BRISTOL

| | |
|---|---|
| Enumerated in Bristol .. .. .. .. | 233,000 |
| Enumerated in one of six neighbouring Counties .. | 45,000 |
| Elsewhere in England or Wales .. .. .. | 55,000 |

There is no information to complete the study and state the total of Bristolians who were overseas or in Scotland in 1911.

An alternative method is used for studying recent changes, based on an examination of Ministry of Labour Insurance books. It is found that in 1938 13 per cent. of insured males had books which had originated outside the Bristol area, and much interesting detail results from minute examination of the districts from which the migrants came and the trades they entered. It is found that there was a high proportion of unemployment among migrants, and fear is expressed that owing to their concentration in particular industries, which may not have a prosperous future, the industrial organisation of Bristol may be unbalanced; the older industries of Bristol have not made much progress in recent years.

The latter chapters of the book, by Mr. Grebenik, deal with the problems of fertility and future population. Records are available in Bristol of a kind that have not usually been preserved, and there is some detailed work of special interest to students of population. Since the main chapter was communicated to the Royal Statistical Society in 1940 and discussed, it is not necessary here to describe this very technical study.

It will be realised that this work is mainly statistical, but it touches on many topics of interest to realistic economists, and it is valuably suggestive to post-war planners.

A. L. BOWLEY.

*Keynesian Economics*. By MABEL F. TIMLIN. University of Toronto Press. Great Britain: Humphrey Milford. vii + 198 pp. $2.75.

When a book is as good as this one, there is a danger that the reviewer, reading with exceptional interest and attention, will in criticising it apply a far higher standard, and find more things to wish slightly different, than in a book of lower quality. In a sense this book is, I believe, a statement of the author's faith. She has found in the General Theory something which is both intellectually fascinating in itself, and hopeful for mankind, and she has wished to restate it in her own manner. The result is a book which makes plain on every page the high competence of the author and the very great and sustained care she has used in writing it. In its possession of clear-cut purposes and method, in its detailed thoroughness, and in consistency, good architecture, and exactness and clearness of statement it is outstanding.

The author first sets down the social implications of Lord Keynes' discoveries. They have shown that we are *not* faced with a choice between personal freedom and full employment. The fact that liberal democracies have in recent decades suffered from heavy unemployment does not constitute an argument having any force whatever for setting up in peacetime a state wherein each man's job and the composition of his real income is prescribed for him. We can have freedom *and* prosperity. Her exposition of the theory itself, compared with Lord Keynes' own, pays somewhat more attention to making clear the basic and formal logical frame, and to emphasising its nature as a system of interdependent relationships, the values of whose variables are determined simultaneously and mutually. She shows how her method[2] of the "shifting equilibrium" allows such a set of mutually compatible values to be supposed to hold during a short interval wherein the forms of the determining functions are assumed to remain constant, and how at the instants which separate one such interval from another the changes of expectations and desires, which have accumulated unrelieved during the interval, are allowed to take effect, altering the forms of the functions which will be in force during the next interval. Her account rests largely on a series of very carefully designed diagrams, in which great pains have been taken to keep in the forefront the notion of simultaneous solution of the system as a whole.

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[1] As a personal matter I record that I was (presumably) enumerated in Bristol in 1871, and not again till some one date in this century when I happened to be there on Census night. I do not know who may have replaced me in intermediate Censuses in Mr. Shannon's Tables.

[2] Taken over from Professor Hicks, from whom she quotes at length.

Professor Timlin's method is based on the Hicksian " week ". Since this concept is the essence of the method of shifting equilibrium analysis, it may be permissible to give one's own interpretation of it. At a given instant, every individual in the economy is simultaneously cut off, for a time, from all further news of the outside world : *except* that he is fully and instantaneously informed of all offers to buy or sell made by other individuals. Thus on the basis of the particular assemblages of information which exist, at the moment of " freezing " of news, in the minds of the different individuals, the process of price-determination in a perfect market goes through. When this process is complete, the prices which have emerged are themselves frozen during an interval wherein the news of ordinary happenings again reaches the individuals, and they are at liberty to act in any way except to make fresh bargains or contracts. At the end of the " week " the news is again cut off while a fresh price-determination takes place ; and so on. In effect, time is broken up into a sequence of alternating segments, one segment being of a kind where ordinary events, but no price-changes, can occur, and where no effect can be given to alterations of plans ; and the succeeding segment being of a kind where price-determination, but no other event, can occur. In these days of almost instantaneous world-wide distribution of market news, there is perhaps little significant difference between this model and the real world. Professor Timlin has perhaps not sufficiently emphasised that this model is no more free from entrepreneurial mistakes than any other model in which there is less than perfect foresight and in which there is a carry-over of goods from one " week " to another, i.e., in which there is investment in durable goods or carrying of stock-piles. In view of this, it may not have been worth her while to introduce the two supplementary models, in which sales of product take place on one day of the week and contracts with " original factors " are made on another : they do not differ essentially from her fundamental model.

Output, consumption, and investment for the " week ", and the rate of interest connecting the current market day with the next, assume those values which satisfy simultaneously a set of four equations, namely the functions of liquidity preference, propensity to consume, the schedule of the marginal efficiency of capital, and the identical equality, *ex post*, of investment and saving. The forms of these functions are conceived to be changed by the events which become known to individuals during any week, and these changes of form take effect on the next following market-day. In this way the system follows a path which under certain assumptions will be a steady approach to a state of stationary equilibrium, and under others will be a cyclical oscillation or a series of non-regular movements. The greater part of the book is concerned with the nature of the three functions and their probable shape, and with the consequences of their interdependence.

There now exist two theories, each claiming to show the reason for the existence of a positive rate of interest, and the manner in which its level is determined, which are not merely quite different from each other but even totally unconnected : they have, one might say, almost no common ground, except the bare fundamental principle that individuals will try to maximise satisfaction from scarce means. Professor Timlin presents both these theories in turn, and I feel that she has relegated the theory based on time-preference and the technical marginal productivity of the existing assemblage of capital-goods to its proper setting, namely an economy where there is *perfect foresight*. But it is difficult to understand why she has included it in this book at all. Uncertainty, sheer ignorance of the future, and the character and consequences of our reactions to it, are the central basic theme and reason for existence of the General Theory. Professor Timlin certainly did not intend her book as a general treatise on economic theory ; so that even formal completeness hardly requires the appearance in it of this most un-Keynesian of economic doctrines.

Those who read the letter, rather than the spirit, of the General Theory, and even perhaps Lord Keynes himself in the initial stages of his writing, have been tempted to mistake the central position which the rate of interest appears to occupy in the logic and æsthetic of his system for its importance in the real world. Inquiries such as those made (by personal interview and questionnaire) by the Oxford Economists' Research Group[1] in the years before the present war have shown strong evidence that, so far as investment in industrial equipment is concerned, the rate of interest is less potent than orthodox theory, and some interpretations of the Keynesian theory, suggest.[2] The demon who really opens or closes the throttle of economic activity is not the rate of interest, but the marginal efficiency of capital, with everything that this schedule rests upon, that is to say, all the factors which shape expectations, and the " utter doubt, precariousness, hope, and fear "[3] which qualify them. The fact that more than a third of Professor Timlin's book is devoted to the structure of interest-rates is justified, therefore, rather by the difficulty of this subject and the fascination arising from that difficulty, than by its importance, from the point of view of reality, for the theory of employment. It might have been better to retain the order of subjects adopted by Lord Keynes himself, dealing first with the marginal efficiency of capital, then with the propensity to consume, and lastly with the money rate of interest.

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[1] *Oxford Economic Papers*, Nos. 1 and 3.

[2] These inquiries, so far as they touched the question, indicated that the rate of interest does have an importance for housing.

[3] J. M. Keynes, " The General Theory of Employment ", *Quarterly Journal of Economics*, February, 1937.

I did not everywhere feel happy about the author's wording of passages on the liquidity-preference theory of interest. The wording sometimes gives the impression that money has its effect on the rate of interest by being *used in transactions*. The question we really have to answer is " What makes some people willing to *hold*, between them, the total quantity of money in the system, instead of buying income-bearing assets ? " It would be quite false to suppose that a unit of money is passive and without influence on the rate of interest until the moment when it suddenly springs up and buys a bond or other asset. Money is an idler, producing no income : why do people give him house-room ? Because he is a pleasant fellow to have about the house, spreading an agreeable feeling of confidence. The author's exposition of Lord Keynes' answer to this question, especially in dealing on pages 76 to 84 with the $L_2$ balances, as a whole is excellent.

Throughout the book the author is careful to insist that the forms of the functions are largely determined at every moment by the state of expectation which prevails. It is indeed this idea and the fundamentally important and far-reaching implications which it carries that mainly distinguish Keynesian and modern Swedish economics from what went before. It might have been well, perhaps, to point out that, while no doubt a fully specified assemblage of sets of expectations, one set for each individual, leads to a unique set of forms for the functions, the converse is not true : for any one set of forms of the functions there are infinitely many different assemblages of expectations which will all lead to it. The reactions of individuals to a failure of events to conform to their previously-held expectations will depend on what these expectations were : consequently we cannot say that a given set of forms for the functions will lead to a uniquely determinate course of events, in which expectations are revised in the light of actual events in a uniquely determinate way : we cannot tell what the path will be unless we know, not merely the initial set of shapes of the functions, but the initial assemblage of expectations and the " schedule of reactions " of every individual to all possible types of disappointment.

G. L. S. SHACKLE.

*Road and Rail.* By GILBERT WALKER. George Allen & Unwin, Ltd., London. 1942. 236 pp. 12s. 6d.

Mr. Walker has written a very competent study of road and rail competition for goods traffic (passenger problems are excluded). He begins with a pithy summary of the legal framework of railway charges, including a useful note on the " agreed charge ". Then comes a description of the road transport market, all the more valuable because it contains a good deal of information on prices which is not easily to be found elsewhere (the outstanding gap being the treatment of the clearing house system on which it would be very useful to collect comprehensive information) ; this is an outstanding addition to the literature of road transport. It leads up to the main task of the book, the analysis of road and rail competition.

Mr. Walker believes that the railways are economically best suited to carry the bulk traffics, and road transport the smaller consignments ; railways the dense traffics, and road transport the traffic on light routes ; and that instead increasingly up to 1939 road transport was taking bulk traffic on dense routes and leaving to the railways small consignments on light routes. Why was this ? Not because wages in road transport were lower than wages on the railway, as used to be alleged. Nor because road transport is too lightly taxed—it paid far more than the roads cost—though interested parties make the fantastic demand that commercial transport alone, excluding the private car, should pay the whole cost of the road, as if road expenditure were independent of the number and use of private cars. Neither of these factors could be an explanation even if they were true, because they could not explain why road transport not only competes where it should be uneconomic but also does not compete where it should hold the field. The real reason Mr. Walker believes to be the railway rate structure, which charges too much for the traffics the railways are best fitted to carry, and too little for the traffics which are most expensive to handle.

If this was so, why did not the railways alter their rate structure ? There was nothing in the law to prevent them from doing so, since they were not earning the standard revenue the Railway Rates Tribunal would have had to agree. Nor was the law of undue preference relevant. Mr. Walker gets somewhat confused here (p. 171). He proceeds to explain that a railway cannot in practice reduce some rates without having to reduce others, not because of the law, but because traders expect a constant relation between different rates, and the railway loses goodwill if it alters that relation. This is very relevant to some problems, but not to this particular issue. Traders are interested in having a constant relation between the rates on the same goods passing along similar and usually competing routes. But Mr. Walker's point is that the relation between rates on different *goods* (those in the higher and the lower classes) is wrong, and also the relation between rates on dissimilar routes, the dense and the lightly exploited ones. There is no such tradition to prevent the railways from altering the relative general levels of these charges if they so desired. The real reason why railway companies do not make this change is that they do not believe the case that Mr. Walker is arguing : they do not believe that their charges in, say, classes 1 to 6 are too high and classes 7 to 21 too low in relation to costs—on the contrary they are always arguing that on " what the traffic will bear principles " they have to make good profits on the latter to subsidise the former. Mr. Walker may well be right, but the figures on which to make a decision are available only to the companies. Receipts per ton mile are indeed higher on the higher classes, a fact they quote to support their case; but receipts per wagon mile are much lower on those classes, and if we had figures on costs this might well prove to be the truer index.

Mr. Walker's solution is that the system of standard rates, with its implication that a commodity should pay a uniform rate anywhere in the United Kingdom, should be abolished; and that instead each line of railway should charge independently of every other line, whether belonging to the same company or not, the general level of charges on each line depending on the density of the traffic it carries. This would not be a very revolutionary change, since extensive use of exceptional rates on the lines with heavy traffic already brings about a similar result; but he is probably right in thinking that the 1921 Act resulted in excessive centralisation, and that some further decentralisation of commercial decisions would be helpful. At the same time, he wants the general level of rates in the lower classes reduced and the higher classes and goods involving transhipment increased; this the railways could have had at any time from the Railway Rates Tribunal without any legislation if they had believed in it. On publication and undue preference Mr. Walker's views are not very clear; at one point (pp. 172–3) he seems to be arguing in favour of freedom to discriminate, but he ends with a fallacious rejection of price discrimination on the ground that it is only justified if no service at all would be provided unless it were permitted (p. 211), when, whatever the merits of railway discrimination, it is certainly possible to justify price discrimination on theoretical grounds in other cases if average costs are falling and the different elasticities of demand such that its use would secure a nearer approximation to "ideal" output (where price equals marginal cost) than would charging a uniform price equal to average cost.

What would be the result of a proper allocation of traffic between road and rail no one can tell without access to figures of costs of different types of traffic which the railways withhold from the public. We are, however, all agreed—at least disinterested people are all agreed—that the principle underlying the allocation should be relative costs and convenience. Mr. Walker believes that its application is most likely to be achieved if road transport competes with railway transport, and states the case with great ability. On the other hand, the railway companies and the large road interests have now agreed on sharing out the market, and their joint committee, the Central Road-Rail Conference, has spent much time during the war working out district by district a detailed share-out for which state approval is to be asked, and is now almost certain to be given, under the blessed name of "co-ordination". Of the various systems of co-ordination—competition between road and rail on the basis of costs, a public transport trust incorporating both road and rail under central direction, and a share-out between road and rail interests—the one which is least likely to secure the public good is the share-out, but it is the one we seem most likely to get.

W. Arthur Lewis.

*Retail Trade Associations.* By Hermann Levy. Kegan Paul, Trench, Trubner & Co., Ltd., London. 1942. x + 265 pp. 15s.

Professor Levy has collected in one volume much information on retail trade which has hitherto been available only in scattered places, and has added a little which he has drawn from a recent survey of trade journals. He has not done as useful a job as he might have done. His discussion follows the usual line of setting out the origins and functions of trade associations, which is familiar ground. What we do not know is which trade associations combine which functions, and a sort of census enlarging and bringing up to date the material in the 1931 Report would have been a most valuable study. (Incidentally, he is not even always aware when the facts in that report are out of date, e.g., on the photographic and chemical trades.) Nevertheless, owing to the preoccupation of economists in the 1930's with counting equations and unknowns, most readers, even most professional economists, will find the book virgin territory, and Professor Levy is to be thanked for reminding us that the study of how our economic system actually works is a proper part of economics.

The basic facts of the situation are easily summarised. (1) Many retailers dislike competition within their ranks and want minimum prices to be observed—the larger retailer and the department store tend to prefer freedom to compete, but the small retailers in particular like price maintenance. (2) The growth of branding has facilitated developments, and a major function of retail trade associations is to persuade manufacturers to fix minimum resale prices; some agree willingly because they believe their sales depend on wide distribution through every possible outlet, rather than on keenly competitive prices; others would prefer quicker turnover through fewer centres, but are held in line by fear of organised boycotts. (3) The associations at the same time insist on "reasonable" margins. These have to be high enough to remunerate most of their members, if not all, and do in fact encourage new entrants to the trade. Some of these are what traders call "poachers", e.g., the grocer taking up a line of business previously handled only by chemists; others are "legitimate", i.e., new entrants who confine themselves to the particular trade into which they are entering. Poachers or legitimates, new outlets reduce turnover per outlet, increase the burden of overhead costs per unit, reduce profits, and make necessary pressure for still higher margins. (4) In consequence some trade associations have learnt that it is no use insisting on higher margins unless they can restrict the number of outlets. "Distance limits" have been imposed in some trades, and sharp scrutiny of the "suitability" of would-be entrants in others. Until the outbreak of war Parliament refused to license shops, and so the fight against poachers and other new entrants depended on organising effective trade pressures.

What are the remedies for this situation? Professor Levy is not very helpful. His attitude to trade associations is definitely hostile; he is not deceived by their fine phrases, and has, as they have not, a clear understanding of the difference between the sectional interest and the general. Neither does he fall for one of our most cherished traditions. The belief that there are too many middlemen is one of the fundamental rights of man, which has been handed down to us through thousands of years of living together; it is a primitive instinct which only the metropolitan housewife has ever been able completely to submerge. Professor Levy does not believe that the remedy is to license shops and restrict entry into retail trade. But what he does believe is not very clear or positive; the book tails off into vague generalisations from which very little more emerges than that there should be some government department to watch the behaviour of trade associations.

In retail trade we have again the familiar choice between trying to make the market work and replacing its automatic mechanism by state machinery. If the state and its officials were all-wise and incorruptible and traders unfailingly honest and courteous, the simplest and cheapest distribution system woud be one in which shops were strictly licensed according to needs which could be statistically computed as so many cubic feet of space per head of the population. Men being what they are, the present reviewer likes to be able to trot around until he finds a shop that suits him, and notices that the housewife also seems to rely on competition to keep the tradesmen up to scratch. Other men think otherwise, and the final outcome will doubtless be some synthesis of competing philosophies.

But, whatever else should be done, surely the first step is to put an end to the arbitrary power which trade associations are able to exercise by boycotting those who do not accept their policies? Price maintenance and trade boycotts were illegal under the old common law of trade conspiracy, and have only received definite recognition within the past fifty years. Prohibition of these restraints would help to free the market, and eliminate many inefficient units from retailing which now shelter behind price maintenance and "reasonable" margins. We ought, too, seriously to consider the advantages and disadvantages of branding, and that large-scale publication of lies and half-truths which passes under the name of advertising, and costs the community over £100,000,000 a year. The world might be a better place if practically all advertising were confined to trade periodicals. The Americans have done some good work in analysing consumer reactions to branded and advertised commodities. Here is a field for some mass observer or team of social investigators in England. From their results we might discover whether that £100,000,000 really performs any socially useful functions at all.

Another team might tell us about the political aspects of free entry into retail trade. In our world of large establishments most men are wage-slaves; retail trade provides one of the few outlets into "independence". How much does the small shopkeeper make, or lose? Is his independence real and of value? Should he be encouraged even if he prove a costly method of distribution? Most of us have a hankering to protect and preserve "the small man", and it is an attitude which makes the future structure of distribution a political and philosophical rather than a purely economic question; since it decides whether we shall follow the U.S.A. in deliberately penalising the chain store, or the planners who want to restrict the number of small shopkeepers. This book will have done a service if only it directs attention to some of the more fundamental problems of our developing society.

W. ARTHUR LEWIS.

*The Theory of Competitive Price.* By GEORGE J. STIGLER. New York: The Macmillan Company. 1942. vii + 197 pp. $3.00.

In presenting this "preliminary edition of a textbook in advanced economic theory" Professor Stigler has earned the gratitude not only of the postgraduate students for whom it was written, but of all students of "advanced" economics. His account of the main theoretical problems is neat and precise, his style lucid, his presentation, which contains an unusual number of apt illustrations from modern economic life, convincing and often elegant.

But the distinguishing characteristic of his book is its unmistakably Knightian flavour. As Professor Knight's teaching, scattered over many learned journals, has thus far solidified into no more than two books (one of them written, if not published, more than 25 years ago) this is a more than welcome supplement. Twice, in chapter 3 and chapter 7, we are explicitly told that the main sections of these chapters are directly based on Professor Knight's lectures and writings and, needless to say, our expectations are not disappointed. But even apart from this, the guiding star of Professor Stigler's inspiration remains transparent throughout his book. For once, costs are *really* treated as foregone alternatives, and the difficulties thereby raised are intelligently discussed and not merely used as a practical pretext for infringing the principle after theoretical lip-service has been paid to it (107–9). The discussion of production is "couched exclusively in terms of anonymous productive services rather than the classical 'factors of production', land, labour, and capital" (114). The awkward problems inherent in having to treat the value of a capital good as composed of the discounted values of all future services are thus evaded. And we are repeatedly reminded that though generic needs are innate in man specific wants are more or less a product of his cultural environment.

The book consists of two parts, a methodological Introduction (chapters 1–4) and a "Theory of Competition" (chapters 5–10). The four chapters of the Introduction deal with The Scope and Method of Economics, Basic Concepts, The Functions of an Economic System, and Fundamental Quantitative Relationships. The second part covers the theories of demand and cost production functions and price formation. At the end of each chapter we find a list of " Recommended Readings ", and (except in the first three chapters) this is followed by a list of " Problems " by the solution of which the student can test himself whether he has grasped the meaning of what he has read.

It is possible to feel that in the methodological chapter I the difference between social and natural sciences and their appropriate methods is rather glossed over. One may legitimately complain that in the section on expectations (95–7) the really essential problem, viz., the nature of " determinateness ", is scarcely recognised. Professor Stigler rightly emphasises that the existence of inventories largely invalidates the rigid distinction between " long run " and " short run " (166–8), but this granted his subsequent time analysis is hardly pushed far enough.

These are, however, minor defects of an otherwise admirable book, and there is every reason to look forward to the " sections on imperfect competition, multiple products, capital theory, and certain other topics " which the author promises us for another day.

L. M. LACHMANN.

*Economic Reconstruction after the War.* By M. JOSEPH and N. KALDOR.
*Private Enterprise or Public Control.* By JOAN ROBINSON.
(Handbooks for Discussion Groups. Published by The English Universities Press, Ltd. 4d. each.)

In the first of these small brochures Messrs. Joseph and Kaldor start by giving reasons why the war should not lead to the permanent impoverishment of this country, and then in the light of the experience of what happened after 1918 go on to deal with the problems of economic demobilisation. Finally, they discuss the future in terms of three desiderata—the maintenance of full employment, social security, and the achievement of industrial efficiency. Having regard to the present state of our knowledge, they believe that the first two of these requirements can be fulfilled, and they indicate how in their opinion this could be done; but about the third they feel more doubt. For the prevalence of monopolistic competition tends to inhibit both the elimination of the less efficient firms and the transmission to the consumer of the benefits which we ought to derive from improvements in industrial technique. Their suggested remedy, the fixing by the State of maximum prices for the articles of mass consumption, to be based on continually revised figures of the costs of the more efficient firms, is stated roundly to be " the only way by which competition can be made to work ". The authors clearly had no space in which to discuss the manifold difficulties to which such a proposal would give rise, and it is to be hoped that if they intend it to be taken as a serious contribution to post-war economic reconstruction, they will develop their argument elsewhere at greater length.

Mrs. Robinson writes with her customary vigour, clarity and persuasiveness in favour of full-blooded socialism. While she faces and provides answers to some of the main objections urged against economic collectivism, her answers will scarcely quell the doubts of the many who, quite apart from any reasons of self interest, dispute the wisdom of so fundamental a transformation of the economic system—not least in view of the position of this country in the world economy. Her contention that " It is only when there is no serious enemy within or without that full freedom of speech can safely be allowed ", and her proviso that *if* the regime is sufficiently secure to risk criticism, independent institutions such as churches, universities, publishing houses, etc., may be " allowed to exist ", will not be reassuring to those who believe that we are fighting to restore freedom to a tyrannised world and not merely to exchange one form of tyranny for another. Mrs. Robinson will have nothing to do with compromise solutions such as the nationalisation of only the major industries or the wide extension of State regulation—" public control " in her words, " requires public ownership ", though she is prepared to concede that " a fringe of small-scale private economy might well survive in a controlled economy provided that it did not threaten to encroach too far ".

Both these pamphlets, which are issued under the auspices of the Association for Education in Citizenship, are well designed for their purpose, which is to stimulate interest and discussion amongst groups of people in the Forces and elsewhere who are thinking about the future of our society.

C. W. GUILLEBAUD.

*Housing Before the War and After.* By M. J. ELSAS. P. S. King and Staples, Ltd. London. 1942. ix + 69 pp. 5s.

After the War, when the formulation of general principles of reconstruction has given way to the elaboration of detailed plans, there is no doubt that the fortunes of the building industry, which in all its ramifications was responsible for the employment of 1½ to 2 million persons in 1939, will prove to be among the most urgent and vital problems of the day. This book is therefore both timely and instructive since, through its analysis of the present housing situation and future demand for houses, it provides valuable data and stimulating ideas concerning future residential building activity. Although short the book attempts to cover the whole of the very wide range of political, social, fiscal and economic problems which is inevitably involved in the housing question. For this reason the author has been compelled to condense into a page, a paragraph, or even a sentence, a complicated problem requiring a whole book to ensure rigorous analysis and unassailable conclusions. Bearing in mind this fundamental limitation there is no doubt that Dr. Elsas' monograph will be invaluable to all workers in the housing field, providing as it does an introduction to the many aspects of the problem and suggesting additional lines of investigation. The value of the book is enhanced by the supporting statistics, careful documentation, and excellent bibliography.

After indicating the essential features of the pre-war housing situation, such as overcrowding, government subsidies, slum clearance, and migration of population, it is shown how most students of housing had predicted an imminent fall in residential building, unless extensive slum clearance was inaugurated. These predictions were based upon estimates of a future decline in the population of this country. The War, however, has created a completely new set of factors, and Dr. Elsas sums up the present situation by rent restrictions, evacuation of population from the towns, the elimination of private building, disintegration of the industry, and the destruction of houses by bombing. Finally, the last 24 pages are devoted to the future outlook, such varied factors being examined as the increase in the adult population during the war, the loss of houses through bombing, the existence of large slum areas yet to be demolished, the possibility of centrally directed movement of population, and such schemes as rent rebates and family allowances. As Dr. Elsas tries to do justice to all sides of an argument, his own general conclusions are not always obvious. But the reader is left with the impression that there still remains considerable rehousing to be done after the war, and that building costs will be sufficiently low to ensure maximum activity in the building industry for many years to come. The book ends with a plea for an adequate standard of housing, the redistribution of industry and population away from the large cities, and, above all, for a detailed plan of action when the war is over.

Since the publication of this book the Minister of Health has disclosed some details of the destruction of houses resulting from the heavy bombing of Britain up to November, 1942. These details, which were not available to Dr. Elsas, present a somewhat clearer picture of the housing situation. Although 2,750,000 houses were damaged by bombs in England and Wales, only 150,000 of these are considered by the Ministry of Health to be permanently destroyed. Against this loss we have to offset 135,000 new houses which were completed after the outbreak of war. As Dr. Elsas points out, there has, however, been normal wastage of houses, which must amount to some 100,000 from 1939 to the end of 1942, and an increase in the number of families over the same period which requires the erection of another 370,000 houses. Thus the ground lost since the outbreak of war amounts, so far, to nearly 500,000 houses. The extent to which this figure will increase by the end of the war depends upon the length of the war and how far air raids on Britain are resumed. It is to be hoped that final victory will not demand the same sacrifice of life as in the last war, but it should be remembered that heavy casualties would tend to reduce housing requirements; the loss of 100,000 men in battle leads to a reduction in the need for houses by perhaps 50,000.

In terms of pre-war building activity it appears from these figures that the effects of the war on the long period housing situation to date have been slight. The ground lost is equivalent to less than two years' house-building at the pre-war rate of 350,000 houses per annum, and predictions of a long period decline in new, as opposed to replacement, building through the operation of demographic factors remain, therefore, broadly unaffected by the war. Continued building on the pre-war scale could only be the result of considerable internal redistribution of population or wholesale replacement of existing houses by means of extensive slum clearance schemes.

The opinion has recently been expressed by the Minister of Health that we might build, in the ten years following the war, at the rate of 400,000 houses per annum. This is a rate of building which would keep 1 to 1½ million persons fully occupied in house building alone. The implications of such building cannot be seen at this stage in the war, but it is possible to examine tentatively what they would be if the war ended to-morrow and we embarked on such a programme. In the decade following the war four million houses would be built. Allowing for (*a*) a gap between families and dwellings at the outbreak of war of about 0.3 million houses, (*b*) 0.3 million houses which would have been condemned as slums but for the war, (*c*) 0.5 million arrears of building during the war, (*d*) an increase of 1 million families during the decade, and (*e*) 0.4 million houses as normal replacements, it is clear that 1¼ out of the 4 million houses built would be in the nature of " luxury " replacements or duplications of existing houses.

In the succeeding decade the situation would be dominated by the fact that, whatever happens to the birth rate, the number of families in this country would be practically stationary; all new building would be replacement. At the beginning of that decade there would be about 12 million houses of which only about 5 million would have been built before 1929; and during that decade one in every twelve would be replaced every year, or, to put it another way, from that day the length of life of a house would be limited to about 30 years! Thus, unless such a lavish scale of housing was introduced, or there were gigantic internal movements of population, about half of the 1 to 1½ million workers engaged on house-building would have to be switched onto factory building, public works building, or some entirely different industries.

But to what extent great clearance schemes, redistribution of population and industry, and therefore building, can be carried out after the war depends on the National Wealth, and the claims on that wealth. Unfortunately, probably for reasons of space, this factor appears as an importunate intruder in Dr. Elsas' book. Nevertheless, the study of housing after the war is, of course, inseparable from the study of the whole economic aspect of reconstruction. Demographic and political factors will play their part, but to make them effective there must be favourable economic conditions. The re-housing of Britain depends upon the level of employment, on the scale of international trade, on the exhaustion of our foreign assets, on consumption of capital and depletion of stocks. If our income is sufficient for all the many urgent demands which will be made upon it extensive re-housing can be considered, but if our income is low we shall be compelled to repair and adapt existing buildings to our needs and restrict new building to a minimum. Research into public health before the war, for instance, led M'Gonigle and Kirby to conclude that food and not housing is the main factor in public health; this suggests that it would be a mistake to devote the national income to re-housing if it impaired adequate nutrition. Before a housing policy can be framed it is thus essential that a survey of the whole economic condition of the country after the war should be underatken and an examination made of the standard of living possible under the new conditions. A standard of housing commensurate with the standard of living could then be ascertained and translated into a realistic housing programme.

Dr. Elsas could not, however, have examined the many economic problems of reconstruction in the space available to him, and it was, therefore, unavoidable that, to this extent, his analysis of future housing policy should be incomplete. But in spite of the limitations which must in consequence be imposed on his conclusions he has certainly succeeded in providing an excellent survey of the housing problem, and in making a strong case for immediate detailed planning of post-war housing.

H. W. Robinson.

*Australia's Dying Heart.* By J. H. Pick. Melbourne University Press. Oxford University Press. 1942. 90 pp. 3s.

People who talk about deserts in Australia are seldom popular. Nearly 40 years ago the late Professor J. W. Gregory achieved notoriety by a volume entitled *The Dead Heart of Australia*, and the work of Professor Griffith Taylor during the last three decades has been equally unpalatable. Australian politicians apparently do not like to be reminded that more than one half of the continent has less than 10 inches of rainfall annually, with a large part receiving less than six inches. Still less do they like to be told that unless stern measures are taken the desert will spread, as it has done in other parts of the world, in times long past and within living memory, when human occupancy has misused the land surface.

To a large extent, perhaps, the average Australian is not very interested. Over half the population lives in the great coastal towns, enjoying comparative prosperity from trade and subsidised industry. If the " Inland " is visited at all it is most likely in a good season, when the annual flora gives the semi-desert an appearance of rich steppe-land. Those who cry " desert ", " rabbit plague ", and " erosion " are thus not very attentively heard.

If the rainfall really were ten inches every year—or even six—the problem would hardly exist. But it is well known that an *average* of six inches may mean perhaps two or three years in every decade with a fall of eight or ten inches, while the remaining years get four, two or even nothing. In a good year the saltbush country will carry 40 sheep to the square mile, but during the drought years ten would be a dangerously high figure. Moreover, the sheep country is also the home of the rabbit, and over-grazing by sheep and rabbits during a drought may well destroy the perennial flora to such an extent that several years may elapse before it can recover sufficiently to carry stock again. Meanwhile, however, wind erosion has played havoc with the surface soil, and intense water erosion accompanying the heavy rainstorms which break the drought may ruin the land beyond the possibility of recovery.

Other parts of Australia—areas with up to 20 inches of rain—have their erosion problems, but this book is mainly concerned with the " Inland ", the " under ten inches " area. Its author is a sheep farmer from the saltbush of South Australia. It is not surprising therefore that he has an axe to grind: against politicians, against the rabbit, against the system of land tenure and the financing of the wool industry. Nevertheless, despite occasional lapses into over-emphasis, the background of facts is solid and well marshalled, and the suggested remedies are worthy of attention, especially that relating to rabbits on pp. 41–2, the idea of flexible rent on pp. 58–9, and the establishment of a permanent Soil Conservation Service, free from political control, pp. 66–71. The two greatest problems are the elimination of rabbits and the prevention of over-stocking. If only these two can be solved before erosion and the destruction of the saltbush have gone too far, the recuperative powers of Nature may be able to arrest the creeping spread of the desert.

S. H. Beaver.

*England's Service.* By Sarpedon. London: Macmillan. 1942. 177 pp. 7s. 6d.

It is encouraging that books directed towards informing the general public on politico-economic issues are increasing in number and improving in quality. This is such a book. What is desirable here may perhaps be summarised as: giving information on matters on which it is important that people should be informed; doing so in a generally well-balanced way; and being understandable and interesting in a way that does not put too great a strain on the general reader.

On all these scores *England's Service* is a good pass. It seeks to drive home a realisation of our dependence on world commerce. And its insistence that this is the work of men and not just the bounty of nature is timely to-day when so many indulge in facile comparisons with the naturally enormously endowed Russia. The picture drawn is also admirably clear; and, so far as it describes and interprets the past, it is broadly shrewd and true.

Such books have, of course, inevitable limitations. The author notes that his exposition " must be brief and therefore devoid of some really desirable qualifications ". And where shading is omitted the impression of overstatement can hardly be avoided. It is thus rather a shock to be told *simpliciter* that "The advancement of its own national interest is *ex hypothesi* the sacred duty of the State": or that the power of "capitalist monopolists" has now gone (p. 85). And the statement that Britain's income "is derived, very roughly, as to one-third from overseas investments, one-third from commerce and one-third from industrial pursuits carried on in this island itself" (p. 21)—such a statement requires at least interpretation.

These inevitable limitations are, however, emphasised by what is in itself a merit in *England's Service*. The book does not merely summarise accepted doctrine. It puts a case for the future. But here arises a real danger to the less initiated public. For the author's more individual opinions may be accepted as of equal credit with the more established currency. And this danger is the more formidable because most of the forward-looking opinions will seem to many merely academic economists to be singularly naïve, surprisingly so in one who can pass such shrewd judgment on past institutions.

Briefly, the plan put forward proceeds on the assumptions that the international money system based on London broke down finally and entirely between the wars; and that the ability of governments to secure progress through treaties and inter-governmental administrations has proved equally mythical. Here there is again definite over-statement. But, granting this, the only hope left is represented as being a return to the methods of the pre-1914 era, though return in an improved form. The task of controlling the amount of goods crossing the frontiers is to be committed to international cartels for each commodity: their exchange is to be managed through the international markets of the London merchants. And if we ask on what principles the exchange ratios are to be controlled, we are told the tests must be a mixture of "justice" as interpreted by the courts and international private law, and the aim to secure full employment.

Most academic economists, knowing their history, will be distinctly uneasy about the suitability of international cartels for these powers. But their main alarm will spring from the suggestion that such bodies should be considered capable of dealing with the essential *economic* question—how much of *each* commodity should be produced relatively. Little comfort will result if the London merchants are thrown in to help—especially if it is true that all traces of an international money have gone for good, and that national governments can never recreate one. For the relatively objective money of the 19th century did give the individual at least some slight scope for buying what he wanted: and that is certainly better than merely accepting what London's "international marts" decide.

But in fact, if we believe in democracy, every citizen must have *some* source of control over the relative amounts our scarce resources are to produce. And most academic economists are fully convinced that, faced by the present array of associations and monopolies, the citizen's hope of such democratic control can grow only through his government. The trouble has been that governments have so far not been sufficiently able and willing to represent him.

In any case, the "City of London's Service" (surely a more exact title for this book—England, let alone the British Commonwealth, deserves wider prospects) was based in the 19th century on Britain's economic leadership, and the British Navy behind it. But to-day such a peaceful basis can be restored only through co-operation between national governments, and the creation of an adequate international police. On that foundation there is no reason why a new international commerce and money should not again arise.

In sum, as an interpretation of the past this is a shrewd, well-informed and stimulating book. It will fill a possible gap in the layman's knowledge. But there is a danger that the uninitiated will be led to accept a rather dreadful simplification of the deeper economic and finally moral questions we have to solve. The encouragement of such evasions is to be deplored.

A. L. MACFIE.

*Brazil, 1940–41: An Economic, Social and Geographic Survey.* Ministry of Foreign Affairs, Rio de Janeiro, Brazil. 1941. 382 pp.

*The Mineral Wealth of Brazil.* By JOSÉ JOBIM. Rio de Janeiro. 1941. Price 20 milreis. U.S. $1.50.

The two volumes under review were both compiled before Brazil joined the Allies, and hence do not deal with the most recent war developments, but they contain abundant evidence both of the great interest of Brazil as a potential supplier of scarce goods (such as rubber and various minerals) to the common cause, and of the considerable adjustments made to war conditions and carried out largely with reference to the war needs of the Allies, even before Brazil became a belligerent. But it is from the long-term point of view that these lengthy volumes deserve to be studied. They contain the basic geographical, economic and social facts about a country which undoubtedly has vast, but as yet undeveloped, potentialities, and which is at that interesting stage of preparing for a big step forward in the direction of industrialisation. Special interest is also attached to the fact that Brazil is faced by peculiar difficulties and problems—such as the lack of certain raw materials essential for industrial production and the backwardness of transport—which will call for novel treatment if the country is to claim a place amongst the leading industrial nations of the world.

The first volume to be reviewed is a year-book, compiled by a body of experts, collaborating with the primary object of presenting to the outside world, in the first place, a survey of Brazilian resources, and, in the second place, an account of the reorganisation that has occurred since 1930 under the Vargas Administration. The climate, soil, nature and distribution of the population, immigration, colonisation, public health, armed forces, foreign policy, education, labour problems, Civil Service organisation, primary production of all types, fuel and energy resources, manufactures, foreign and domestic trade, finance, and transport and communications are all exhaustively discussed. There are also rather scrappy " historical notes ", purporting to review political and constitutional developments, but the information given is unsystematic and quite insufficient to provide any clear idea of either the past or present political and constitutional situations. It is as a store-house of material on the resources and economic progress and policy of Brazil that the volume must be judged. From this point of view it can be said that it provides a mass of information, in a somewhat indigestible form. There are no diagrams or maps (apart from those illustrating purely geographical features) and the statistical tables fail to give a clear summary or picture of economic tendencies and the present position. In other words the volume forms a useful reference-book for those interested in particular commodities, industries or other economic features, rather than a vivid picture of Brazilian economy.

The second volume to be reviewed gives a fuller account of Brazilian mineral resources than the section (written by the same author) devoted to this subject in the year-book. In particular, it contains general surveys of the chief types of minerals, which sum up clearly the main features.

The year-book's comparison of the effects of the present war, with those of the first world war, is of great interest. Then, as now, there was great pressure in the direction of industrialisation, but the stimulus proved merely temporary. Brazil was not in a position to maintain the progress made, so that after 1918 there was retrogression towards the former dependence upon agriculture, except that agricultural production tended to become more diverse.

The inauguration of the Vargas Administration proved a turning-point. Social legislation started in earnest. A Ministry of Labour was created, and between 1931 and 1941 no less than 160 laws were passed to regulate and protect labour, including the provision of marriage loans and other measures to encourage population growth. There has been a marked tendency to import raw materials and semi-manufactures and undertake final manufacture in Brazil, instead of importing finished goods—between 1938 and 1940 industrial production increased by 25.7 per cent. and iron and steel and metallurgical manufactures increased by 39 per cent. Brazil possesses no less than 22 per cent. of the known iron-ore resources of the world. At the same time Brazilian exports of manufactures have increased substantially. The country is now clearly in a better position than in 1914 to take advantage of the opportunities offered by the war, though still hindered by the difficulty of securing the necessary imports of plant and equipment, and above all by the lack of adequate transport and communications. For instance, there is ample coal in the South, but no means of transporting it to the manufacturing centres. There are also great, undeveloped, hydro-electrical resources, and it is interesting to note that, since these volumes were written, it has been announced (i.e. in March, 1943) that the petroleum resources of Baia (where the first well, at Lobito, started production in January, 1939) have been proved sufficient to supply Brazilian needs for many years to come. But much remains to be done before Brazil can take full advantage of her resources and potentialities. As the year-book naïvely says, the greatest impediment is still the " habit of not wanting to make researches " !

VERA ANSTEY.

*The Economic Problem of India.* By T. N. RAMASWAMY. New Book Company, Bombay. 1942. 308 pp. Rs. 10.

Mr. Ramaswamy writes with vigour and obvious sincerity, and visualises clearly many of India's most vital economic deficiencies and problems. In particular he emphasises the serious problem arising out of the rapid growth of population, unaccompanied by any corresponding increase in agricultural output, let alone any substantial increase in industrial employment. Far from repeating the widely-held fallacy that India is economically exceptionally well-endowed, he maintains that she suffers from inherent natural deficiencies which will permanently prevent her from attaining a place amongst the economically most advanced and wealthy countries of the world. Moreover, according to Mr. Ramaswamy, poverty tends to increase rather than to decrease. What is the cause of this highly unsatisfactory state of affairs ? What policy should be pursued ? How can production be increased and a reasonable standard of life be secured for the rapidly expanding population ?

Mr. Ramaswamy's diagnosis is that the prevailing poverty is fundamentally due to the impact of the Western competitive system on the formerly essentially stable and self-sufficing Indian economy. It has proved impossible to make the necessary social and economic readjustments, with the result that, although India possesses unused resources (in the form of land and labour—Mr. Ramaswamy completely ignores capital), and needs what could be produced by them, under competitive conditions it is " unprofitable " to extend production—for instance to the large area, classified as " cultivable waste ". His remedy is to ignore what he calls " competitive costs ", to plan " production for use ", adopt barter exchange within the country, and to return to self-sufficiency—he goes so far as to advocate regional rather than national self-sufficiency—by isolating the Indian from the external market. This, he thinks, will enable large new areas to be brought under cultivation, permit more intensive cultivation (increasing output per acre) and —above all—will restore stability (which he confuses with security) to the whole system. He condemns the various measures, such as protective tariffs and subsidies, so far adopted to stimulate industrialisation, whilst he considers more fundamental social reforms—such as the adoption of birth-control—impracticable and even, possibly, unnecessary and undesirable. In particular he fears the effects of a differential birth rate if birth-control should be adopted by the higher classes.

It is true that the problem of poverty in India has so far proved intractable, and it is all to the good that a fresh mind, untrammelled by the orthodoxies of the past, should be brought to bear on the problems involved. Mr. Ramaswamy's outlook certainly leads him to emphasise a number of important points—such as India's inherent economic deficiencies; the difficulty of introducing social reforms on a large scale, within a reasonable time; the need to secure an appropriate " way of life " suited to the country and the people, instead of aiming merely at providing the means of livelihood; and the need to avoid in the economic sphere, as also in the political and constitutional spheres, mere imitation of the West. In all this he is eminently realistic. But when it comes to remedies, he completely ignores the most elementary economic principles, in particular the question of real, as contrasted with money costs, and the inescapable facts connected with the division of labour, specialisation, large-scale production and modern technique, which alone have made it possible to support the 389 million people enumerated at the last Indian Census (in 1941). It has been well said that the principle that the international division of labour is a source of prosperity and wealth is the only one that is common to Marxian and liberal economists. But, here again, Mr. Ramaswamy is a heretic. The fact is that he objects to the developments which have occurred in India since the early 19th century. This is understandable. But the clock cannot simply be put back. The recent past provides us with the data with which we must now reckon. The increased (and still increasing) population cannot simply be wished away. This is not to say that there is no room or hope for a " Plan " for India. On the contrary, it is practically certain that national and international planning have come to stay, and when peace comes it may well be that internationally agreed regulation of trade, certain types of production, and capital movements may provide the remedy for those excessive fluctuations in prices and in general economic activity which, in the past, have been the real bugbear of India's international economic dealings. Moreover, the war is presenting India with an opportunity to industrialise, and in general to modernise in many respects her economic equipment, organisation and technique, which, together with the revolution with regard to foreign indebtedness in India—India has already repaid the bulk of her sterling debts, at the same time as accumulating large sterling resources in London (over £300 millions at the beginning of 1943)—may completely alter her economic position and prospects, when the war is over.

VERA ANSTEY.

*Two Manuscripts by Charles Davenant.* A Reprint of Economic Tracts edited by G. HEBERTON EVANS, Jr., Professor of Political Economy, The Johns Hopkins University. With an introduction by ABBOTT PAYSON USHER, Professor of Economics, Harvard University. Baltimore: The Johns Hopkins Press. 1942. xii + 108 pp. $1.75.

In July, 1696, King William wrote to the Duke of Manchester, then Secretary of State: " If you cannot devise expedients to send contributions or procure credits, all is lost, and I must go to the Indies " (*Parliamentary Papers, Commons*, XXXIII, 94). These are unusual words for an English king, and sound almost incredible in the mouth of so valiant a monarch as the first Orange-Nassau on the English throne. But the crisis of those days was a matter of no ordinary magnitude. The monetary demands of the Treasury rose to a climax at the very time when the monetary system of the country was completely disorganised. While, according to the Statute Book, silver was legal tender, economic practice based all valuations and transactions on gold. The white metal was undervalued at the mint and dropped out of circulation, and this was only the first step towards its leaving the country. Yet cash was badly needed for supplying the armies locked in deadly struggle on the battle-fields of the Continent. In this situation three suggestions were put forward: the first came from William Lowndes, Secretary to the Treasury. He wished to raise the mint price of silver to the height of the market price. The second was in complete opposition to this idea; it came from John Locke. As a strict metallist, he advocated a general recoinage on the old basis. While Lowndes represented the empirical, Locke defended the strictly theoretical point of view. The third suggestion was due to Charles Davenant and is set forth in the two memorials which are here for the first time published. This is not the place to say more about them. It may suffice to quote a sentence which characterises the drift of the argument: " Whoever . . . proposes to make any Regulation of the Coine must beginn with looking nicely and narrowly into the universall Posture of our Trade " (8). It is gratifying to see how Davenant endeavoured to avoid the opposite vices of empiricism and doctrinairism.

It is rather to be regretted that the editor has not given more help to the reader, but reprinted the MSS. as they stand, without even obviating their many disturbing slips of the pen. To give one instance. On p. 97 we read: " Moneyed men must be invited by large Encouragements to let their Hoards see the Light, and be dispersed abroad even to the remoter Countreys." But what Davenant means are not the remoter countries, but the remoter counties of Britain, where at the time a disagreeable shortage of cash made itself felt. Here, surely, and in similar cases, a footnote should have informed the reader of the difference between word and meaning.

Yet, these are petty matters. The value of the publication lies in the new light it sheds on Charles Davenant, and on the mercantilistic school to which he belonged. As far as Davenant himself is concerned, it affords very welcome information on his views on credit circulation which do not figure prominently in the writings contained in the " Works ". But the little book may also contribute something to a better understanding of mercantilism in general. The old stereotyped opinion which still prevails that mercantilism was simply erroneous, that it rested on a confusion of private riches with public wealth, is effectively refuted by words like the following: " Numbers of men, Industry, Advantagious situation, Good ports, skill in Maritime affaires, with a good Annuall Income from the Earth, are true and lasting Riches to a Country; But to put a Value upon all this, and to give life and motion to the whole, there must be a quick stock running among the people, and alwayes where that stock increases, the Nation growes strong and powerfull; and where it visibly decayes, that decay is generally attended w/i/th publick Ruin " (72). Davenant knew as well as any modern economist that " the Originall value of every thing is according as it is necessary to the Common uses of Life " (7).

W. STARK.

*The Theory and Practice of Central Banking, 1797–1913.* By E. VICTOR MORGAN. Cambridge University Press. 1943. xii + 252 pp. 15s.

This book is something more than just an addition to the literature of the Bank Restriction period; it is a historian's appraisement, dispassionate and instinct with perspective, of the progress of monetary theory as far as concerns central banking. It brings together a succinct account of the influences which developed and shaped the Bank of England's activities, a survey of the policies evolved, and an analysis of the fundamental theories which arose from and reacted on them. The fusion of these three elements inevitably recalls *A Century of Bank Rate* and *The Pound Sterling*, but Mr. Morgan's purpose is at once wider than Mr. Hawtrey's and more intensive than Mr. Feaveryear's, so that his book will find its own niche.

Of the purely historical sections it is not necessary to say much here. Their rigid compression sometimes brings the reader to the verge of mental indigestion, but the cumulative picture built up is important for a right assessment of the currency policies discussed. Nor is Mr. Morgan content merely to quote again the customary sources; he tackles them with a shrewd faculty for synthesis, so that his reconstruction of the balance of payments for 1805–1821, and his summaries of the effects of railway finance and the Franco-German War Indemnity on the money market, are real elucidations of difficult problems.

On the policy pursued by the Bank in this changing environment there is not much, at this time of day, which is new to say. The available evidence has been traversed so often that a new writer can do little more than hazard a fresh choice amongst the known alternatives. Mr. Morgan deals justly with the Bank's conservatism and its reluctance to accept responsibility, and he is ingenious in his reconstruction of the darker mysteries of the Court's decisions. If in some of these speculations he is not wholly convincing—as in his dating of the genesis of the Palmer rule, or his suggestion (p. 92) that the regulation of the note-issue by reference to the exchanges was new in 1825—every student of the voluminous evidence is free to form his own opinion.

Partly because the latter half of the book's period is less exciting for the monetary theorist, and partly because it has already been well worked over by Mr. Hawtrey and Mr. Sayers, Mr. Morgan's treatment of the Great Boom, the Great Depression, and the early nineteen hundreds, though excellent examples of compressed economic history, will not be of the same interest to the specialist reader.

It is apropos the Bullion Committee that the book makes what, for one reader at least, is its most original and stimulating suggestion. After explaining how, on the ultimate issues, Bullionists and Anti-Bullionists held the same views, Mr. Morgan accounts for the persistence and success of the opposition by the fact that practical men realised that the deflation needed to give effect to the Committee's conclusions could not be faced in the middle of a war. Parliament and public opinion were thus faced with a choice between a logically unanswerable theory requiring action of Quixotic impracticability, and an anti-theory based largely on the feeling that doctrinaires requiring such action could not possibly be really correct. This antithesis reappears, only slightly modified, in the Currency and Banking controversy, although by then, of course, the failure of Attwood's proposals for reflation in 1819 had largely prejudged the issue in a sense opposite to that of 1810.

As to the essentials of monetary theory, Mr. Morgan is zealous to debunk the Restriction controversy of any merit. Although, as his final summary shews, the stimulus given to abstract theory by the experiment of inconvertible paper did induce a refinement and clarification of the theories of domestic and international prices, and of the rate of interest, he will allow no more than " a restatement of theories all of which had been stated before, and many of which were a century old ". The writings of Professors Viner and Wood, and of Mr. C.-Y. Wu, which Mr. Morgan does not mention, suggest that in this verdict he is a little unkind to the controversialists, who were groping their way through the obscurities left by the venerated Adam Smith, and who, after all, were more concerned to deal with practical issues than to elaborate the theories on which they based their recommendations. But while the monetary specialist will continue, unrepentant, to give fascinated attention to the period 1790–1844, he will undoubtedly find Mr. Morgan's penetrating summaries among the most useful tools which have so far come his way.

J. K. HORSEFIELD.

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# J. S. Mill and J. E. Cairnes

By George O'Brien

## I

It is a matter of common knowledge that John Stuart Mill entertained a very high opinion of his Irish contemporary John Elliot Cairnes. This high estimate is enshrined in the preface to the sixth edition of Mill's *Principles of Political Economy*. Particulars of the friendship which prevailed between these two famous economists have come to light by the discovery of a long correspondence between them, beginning in 1859, when Cairnes was appointed professor of political economy in Queen's College, Galway, and continuing until 1873, the year of Mill's death. Cairnes kept all Mill's letters to him and also kept copies of all his own letters to Mill, so that it is possible to trace the relations between the two correspondents without any gap on either side.[1] Six of Mill's letters have been printed (apparently from Mill's drafts) in *The Letters of John Stuart Mill*, edited by Hugh S. R. Elliot, published in 1910. During the earlier part of the period covered by the letters Cairnes lived in Ireland while Mill divided his time between London and Avignon. After 1865, when Cairnes settled down in London, he met Mill so frequently that their intercourse was verbal rather than written, but, during Mill's absences in Avignon, the correspondence was resumed. Both writers emerge with credit from the letters. Cairnes was the younger and less eminent man. His attitude towards Mill was one of great veneration and respect. In a letter to his brother-in-law, Professor Nesbitt, dated May 9th, 1859, he describes Mill's condescension after a meeting of the Political Economy Club. " Tooke, with whom I have been immensely taken, was almost fatherly. He invited me to dine with him at the Athenæum. But my greatest triumph has to be recorded yet. On leaving, J. S. Mill happened to be going the same way and actually took my arm and walked down Pall Mall with me. You would suppose from his manner that *I* was conferring the compliment upon *him* ". Mill fully reciprocated the younger man's admiration. Expressions of praise for Cairnes's work, of encouragement, of anxiety and good advice during his frequent illnesses abound in his letters. On April 6th, 1872, he wrote from Avignon: " I have occupied myself with examining old letters to make a clearance of those which are no longer worth keeping. In this last operation I have re-read, with much pleasure, many old letters of yours, nearly all of which, both for the marks of friendship they are full of, and for the intrinsic worth of their contents are much too valuable not to be still treasured up. I hope to receive many more of them before I die."

This correspondence was discovered recently in a box which had lain unopened for many years in the house of Professor Cairnes's son, Mr. F. E. Cairnes, in County Dublin. A daughter of Mr. F. E. Cairnes married a kinsman of J. S. Mill, Mr. C. K. Mill. The publication of this description of the correspondence is due to the kindness of Mr. Cairnes and Mr. Mill. In addition to the series of letters to and from J. S. Mill the box contained a number of letters to Cairnes from other economists—for example, Pryme, Jevons, Thornton, Bagehot, Cliffe Leslie, Tooke, Newmarch and Lord Overstone. None of these letters, however, contains material of interest or importance and no copies of Cairnes's replies were preserved. The outstanding discovery in the box was, however, a parcel addressed " J. S. Mill, Esq., India House," and labelled, in James Mill's handwriting, " Mr. David Ricardo's Manuscripts", which, when opened, was found to contain a number of manuscripts and political notes by Ricardo, some letters from Malthus to Ricardo and a complete set of letters from Ricardo to James Mill from 1811 to 1823. This valuable collection of manuscripts has been entrusted to Mr. Piero Sraffa, who is editing the works of Ricardo for the Royal Economic Society.

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[1] [It is hoped that at some later date it will be possible to publish the whole of this correspondence, perhaps as a separate volume in the edition of the letters of John Stuart Mill which is being prepared by the London School of Economics.—*Ed.*]

The letters which passed between J. S. Mill and Cairnes during the fifteen years of their correspondence deal with a very wide range of topics. It is possible to gather from them Mill's candid opinion of some of his distinguished contemporaries. In Cairnes he evidently felt that he had an intimate friend whose discretion he could trust. The following extracts from Mill's letters constitute a sample of the type of comment with which they abound.

THE CAMBRIDGE CHAIR: At the time of the filling of the new chair of political economy at Cambridge, Mill's view about the candidates was as follows: " It will be a disgrace to Cambridge if Macleod gets the professorship. Fawcett's qualifications I shall be better able to judge of after the publication of his book. But at any rate I am very glad that there is a candidate of whom you are able to speak so highly as you do of Mr. Courtney " (March 25th, 1863). Fawcett was elected on November 27th, 1863, receiving 90 votes against 80 for Mr. J. B. Mayor of St. John's College, 19 for Mr. Leonard Courtney, and 14 for Mr. H. D. Macleod.[1]

W. T. THORNTON: " Thornton is a person I particularly respect and like. In perfect candour, sincerity and singleness of mind few men come near him " (October 3rd, 1864).

T. E. CLIFFE LESLIE: In the course of a correspondence relating to Cairnes's intention to resign his chair in University College, London, Mill expressed the opinion that " Leslie is, next to you, by far the fittest person I know to fill the place, among those who would take it " (July 23rd, 1867). Five years later the same subject was again under discussion. Mill wrote: " Leslie seems to have much the strongest claim in reference to this special subject and I should rejoice if he were successful because I esteem and value him, and because I know no one on whom a little worldly success would have a more beneficial effect " (May 15th, 1872).

C. BRADLAUGH: " I feel an interest in Bradlaugh, not merely because of the religious cry raised against him, but because he is as willing to stand up for opinions disliked by the working classes as for those by which he might hope to recommend himself to them; witness his earnest Malthusianism, his support of Woman's Suffrage and his intelligent and thorough advocacy of Hare's system " (December 4th, 1868).

H. C. CAREY: " As for Carey's book, which I think is the very worst book on political economy that I ever toiled through, the only thing I wish to do with it is to find somebody who will take the trouble to write a detached exposure of it for the American public, on whom I believe it has really some influence. If you know of any person competent and willing to perform this irksome but useful service, it would be a great satisfaction to me to make him a present of my copy " (April 27th, 1870).[2]

W. E. GLADSTONE: " We see in Gladstone the man who speaks from his own convictions and not from external influences. No other Minister would have put forward Ireland any more than reform just at this time when there is no public outcry about it " (December 12th, 1864).

W. S. JEVONS: " I have not seen Mr. Jevons' book, but as far as I can judge from such notices of it as have reached me, I do not expect that I shall think favourably of it. Jevons is a man of some ability, but he seems to me to have a mania for encumbering questions with useless complications, and with a notation implying the existence of greater precision in the data than the questions admit of. His speculations on Logic, like those of Boole and De Morgan, and some of those of Hamilton, are infested in an extraordinary degree with this vice. It is one pre-eminently at variance with the wants of the time, which demands that scientific deductions should be made as simple and as easily intelligible as they can be made without ceasing to be scientific. I look forward with much interest to seeing your notice of the book, which I am sorry not to see in the December Fortnightly " (December 5th, 1871).

The principal subject discussed in the earlier letters was the American Civil War. Both correspondents were, as is well known, enthusiastic supporters of the North. Cairnes's famous book *The Slave Power*, which had a considerable influence on British public opinion, was published in book form rather as a series of articles at Mill's suggestion and was dedicated to Mill. Many aspects of the war were touched upon but one recurrent theme, on which both parties were in agreement, was irritation with *The Times* for its Southern sympathies. Cairnes complained (November 21st, 1861) that *The Times* " almost openly advocated slavery ". Mill, writing from Athens (June 6th, 1862), referred to the reaction beginning to arise against the Southern policy. " The victories of the North have had much to do with the change; the altered tone of *The Times* itself is the measure of the greater chance it thinks there is of the North being successful ". Cairnes wrote to Mill (February 4th, 1863) about the great Exeter Hall demonstration. " Nor," he added, " will it diminish your estimate of its importance that it should have kindled so much fury in *The Times*. What an awful power for evil that paper has become ! Its influence is all pervading; and wherever it reaches and just in proportion as it is powerful, independence of mind, elevation of thought, the sense of justice, honesty and candour decline." Mill replied (February 7th, 1863) " When our whole strength comes to show itself it will be seen to be very great. It is fear of *The Times* that makes public men keep silence. Perhaps they do not overrate the power of *The Times* but they ridiculously exaggerate the danger to themselves of braving it. *The Times* has been very often defeated; but, as it is never wrong without great numbers of people to keep it in countenance, it never suffers any permanent loss of influence. It has passed unhurt through much severer blows than any it has had lately. It lost no credit by its sudden turn round on Bernard's trial, the very day he was acquitted—as strong a case as the sharp turn in 1834 from ultra-Whiggism to Peel Toryism. Such things would have ruined a writer who gave his name; but anonymous journalism can do anything with impunity ". Many similar passages occur throughout the correspondence. *The Times* aroused their indignation daily. One of Mill's letters dealing with the Civil War (January 24th, 1864) contains a curiously prophetic phrase. The occasion was the Schleswig Holstein war. " One is consoled for the madness of all Germany by the progress of the cause of freedom in America and by the wonderful resurrection of the spirit of liberty in France." On hearing of Lincoln's assassination Mill wrote on May 28th, 1865: " What I principally feel is that the death of Lincoln, like that of Socrates, is a worthy end to a noble life, and puts the seal of universal remembrance upon his work. He has now a place among the great names of history, and one could have wished nothing better for him personally than to die almost or quite unconsciously in perhaps the happiest hour of his life ".

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[1] Cf. C. P. Gooch, *Life of Lord Courtney* (1920), pp. 57–61; Leslie Stephen, *Life of Henry Fawcett* (1886); and *Dictionary of National Biography*, Vol. VI, p. 1117.

[2] cf. also below, p. 280.

## II

Mill's interest in Irish problems is well known. It is not surprising therefore that many references to Ireland recur throughout the correspondence, especially during the late sixties when Irish agrarian, ecclesiastical and educational reform attracted attention. On one occasion (September 1st, 1867) Mill went so far as to suggest that a possible method of solving " the Irish difficulty " would be for England, Scotland and Ireland to join the American Confederation as three States of the Union. Mill's *Principles of Political Economy* contained numerous references to the Irish agrarian question. The sixth edition, published in 1865, contained much additional Irish material, practically all of which was, according to an acknowledgment of the author, supplied by Cairnes. In 1866 the land question again engaged the attention of the two correspondents. In a letter of January 6th, Mill criticised Cairnes's letter to *The Economist* proposing that tenants should be allowed to carry away or destroy their improvements as " not potent of much practical good ". " I am disposed to make a much greater claim for the tenant—to demand for him not compensation for his outlay but full equivalent for the additional value which either by his labour or by his capital he has given to the land, to be assessed either by a special tribunal or by arbitration." On January 9th Cairnes replied that he did not regard his proposal as a complete solution. It would at least be an assertion of the true principle of the law and would have a powerful educatory force. He described it as a compromise which would probably lead to a greater measure.

In 1867 the question of Fenianism was discussed. On May 20th Cairnes states that he was " much relieved to find that the cry for blood which was raised some time ago on the suppression of the Fenian rising and which for a time found an echo in *The Times* is not going to be satisfied." To which Mill replied on May 26th: " We are not yet safe from the gross blunder as well as crime of shedding the blood of Fenian prisoners. The Government had decided by a majority to hang Burke. About fifty M.P.s, of which I was one, went as soon as possible to intercede with Lord Derby, and at the time of writing I do not yet know the result ". A postscript, dated May 30th, adds: " I kept the letter open to be able to say that Burke's life is spared. But we had to threaten that some of us would go down to Balmoral ". Cairnes replied on June 10th expressing admiration at " the admirable explosion of feeling which the occurrence elicited in England ". Dealing with the land question Mill had said in his letter of

May 26th: " As to Lord Naas's Bill it does so little that nobody seems to wish it to pass except probably himself. I think, however, that it had better pass because it contains the principle of giving compensation for improvements beneficial to the landlord, though made without his consent. But there are symptoms of the Government giving way on this point and if they do the Bill will be a mere authority to lend public money even if ostensibly to tenants yet really to landlords for improvements by which landlords alone will benefit except so far as everybody is benefited by every agricultural improvement ". Cairnes replied in his letter of June 10th that he approved of the Bill because it recognised the principle that the landlord did not possess unlimited rights over the soil. This Bill was later given up by the Government.

In the following year Irish questions continued as the principal subject of correspondence. On February 15th Cairnes expressed himself strongly on Gladstone's policy on education, " The Liberal policy of the future for Ireland is to hand over the country to the enlightened rule of the priesthood acting, of course, constitutionally through the peasantry and their representatives; the single function of the Imperial authority being to enforce the decision of the majority ". He proceeds, " I should greatly prefer separation. That would issue in the first instance in a sanguinary civil war but in the upshot the more progressive of the elements of society would come to the top and in the hurlyburlydom the conditions of progress might be established ". In the same letter he expressed his disagreement with Bright's plan to establish a " peasant proprietorship ", to which Mill replied on March 1st referring to his pamphlet on the land question in which he advocated giving tenants a permanent tenure at a fixed rent or allowing as an alternative the sale by the landlords of their estates to the Government. He added " All the public signs and all the authentic private opinion I have access to tend to show that nothing short of what I propose would now tranquilise Ireland or reconcile the Irish people to the Union, and I am sure that nothing less than some very startling proposal would have any chance of whipping up the languid interest of English public men on the subject and making them feel the critical nature of the situation or exerting their minds to understand it. On the whole I have met with more approbation and not more abuse than I expected ". In another letter, dated March 10th, he explained that his pamphlet was deliberately an overstatement. The object was to strike hard and compel people to listen to the largest possible proposal. " This has been accomplished and now the time has come for discussing in detail the manner in which the plan would work. I do not share your hopes that anything much short of what I have proposed would give peace or prosperity to Ireland in union with England; but if there is any intermediate course which would do so its adoption is likely to be very much promoted by frightening the Government and the landlords with something more revolutionary ". He ended the letter by stating that " The great Irish debate begins to-night ". This was the debate on the motion on the state of Ireland by Mr. J. F. Maguire, in the course of which Lord Mayo outlined the Conservative university scheme to which reference will be made later. On May 29th Mill expressed the opinion that Bright's scheme of peasant proprietorship would do some good but would not give complete security of tenure to the tenants.

Later in the year the Irish Church was discussed. On October 29th Mill wrote: " How weary and sick you must be of the Irish Church and all that belongs to it. The subject is sickening even to me who have not been living in the atmosphere of the abnormal passions which self-interest and bigotry combined have kindled in those who will be losers in one way or another when justice is done. I am impatient to see that worn-out subject dead and buried that the public may get to the practical interests of Ireland, landed tenure and education ". Cairnes replied on November 9th: " The Irish Church is indeed becoming a weariness and heart-sore, and it will be a mercy when it is dead and buried out of sight. The rancour of the clergy and their adherents is only what one might have expected, but I was when in Ireland a good deal surprised to find great lukewarmness on the subject on the part of such priests as I came in contact with. Had they been of the ultra-montane section I should have thought this quite natural, but those with whom I talked on the subject belonged to the opposite side. The truth is, I believe, they all begin to feel that they are parting with a valuable grievance which has not been exploited yet to the extent of its capabilities; and I think something like resentment is felt towards the Liberals who have unexpectedly taken them at their word. I was very much struck with the manifest leaning of one priest, a strong nationalist, towards the Tories, notwithstanding the support he is now giving to the Liberal candidate in his neighbourhood. It showed the purely accidental nature of the alliance between the Liberals and the Catholic Church and how certain the Catholic Church will be, when special grievances are removed, to range itself on the Tory side ".

The general election in which Mill lost his seat is also referred to more than once. In a letter of December 4th Mill, while regretting the result of the election in general, continued: " The unanimity of Scotland and the great Liberal gains in Ulster are of excellent augury as showing that a large proportion of the presbyterians even in the North of Ireland are going in for disestablishment. The lukewarmness on the subject which you observe in the Catholic priests makes the greatness of the gains in the Irish elections more remarkable since it must be ascribed mainly in this to the laity ". On December 12th Cairnes replied: " The Irish elections are in some respects certainly satisfactory more especially the Liberal gains in the North. The energy shown by the priests in Mr. Gladstone's cause has, I confess, surprised and to some extent puzzled me, for in some quarters as, for example, Galway, they seem to have supported the Liberal candidate even in opposition to the lay Catholic feeling. I half fear that they calculate on obtaining from Gladstone other things than disestablishment ". This reference is undoubtedly to denominational university education.

On April 9th, 1869, Mill stated that the settlement of the Church question will open the way for the land question. " It has been, for some time, apparent that when the religious supremacy of protestantism is at an end the presbyterians of the North, and even many Orangemen, will join hands with the tenantry of the South on the land question, and the junction is coming to pass even more quickly than could have been expected. The motion brought forward by Johnstone and seconded by The O'Donoghue for the repeal of the Party Processions Act is a very significant incident ". On April 19th he dealt with the question of the disendowment of the Church stating that " people are very suspicious in Church matters and a large proportion of the warmest supporters of disendowment would have suspected the Government of permitting some employment of the money which they did not dare disclose probably to give it in some form to the Catholic clergy ". On November 16th, 1869, Mill expressed the opinion that the creation of a peasant proprietorship would raise a new agragrian question between farmers and labourers. On December 21st, 1869, Cairnes deplored the re-awakening of Fenianism in Ireland. He rejoiced in the split amongst the Orangemen. He added: " If we were only sure of a really effective Land Bill I cannot doubt that all would come right, but then there is so much in that ' If ' ".

The subject of the largest number of letters in the later half of the sixties was the perennial question of Irish university education. It is difficult in the twentieth century to realise the heat that was generated less than a hundred years ago by denominational education. In 1845 Peel had founded the Queen's Colleges and in 1849 the Queen's University as an attempt to provide university facilities for Irish Catholics on undenominational lines. These Colleges were condemned as " Godless " by the Catholic bishops and were, to a large extent, boycotted by those for whose benefit they had been established. In 1854 the Irish bishops had founded the Catholic University which continued to work amidst great difficulties, unchartered and unendowed. On four occasions, between 1866 and 1873, efforts were made to find a solution for this obstinate problem. In 1866 Lord Russell's Government granted to the Queen's University a supplementary Charter, designed to re-organise it on the model of London University. Degrees would be granted to all comers regardless of their place of education and the Catholic University would receive recognition as an associated College, though without endowment. This scheme aroused the violent opposition of the upholders of the principle of undenominationalism, and was finally brought to naught by a judicial decision that pronounced the supplementary charter illegal. In 1867 Fawcett introduced a resolution to the effect that all remaining disabilities against Catholics in Dublin University should be abolished, to which Mr. Monsell moved an amendment proposing the establishment of a second college in that University. The amendment was withdrawn and Fawcett's motion was rejected by the casting vote of the Speaker. In 1868 Lord Mayo promised to introduce Disraeli's measure for the establishment of a Catholic University. This proposal, which might have solved the problem if it had been adopted, broke down owing to an unfortunate misunderstanding between the Government and the Irish bishops. Five years later, Gladstone, as is well known, was defeated on the Bill by which he proposed to federate the existing Colleges and new Colleges to be established in a great federal University.

Thus the Irish University question defied the best political intellects of the age. To what extent this failure was due to a lack of appreciation by English statesmen of the attitude of the Catholic Church in educational matters, and to what extent to the failure of the Irish bishops to understand the practical limitations to concessions to their viewpoint imposed by British public opinion it is idle to inquire. But there is no doubt that that failure—which would probably have occurred in any event—was rendered more certain by the activities of our two correspondents. Cairnes was a staunch and unbending supporter of the Queen's University and Colleges; and, although not himself in public life, he influenced the minds of some of the most powerful public men of the time. Fawcett, Lowe and Mill were amongst the strongest opponents of the denominational principle; and, in so far as that principle was involved in the controversy over Irish education, they were briefed, instructed and whetted by Cairnes, who was certainly a loyal adherent of the institution which he served. To read his correspondence with Mill on this question is to get a glimpse behind the scenes of the undenominational ranks which were united against any concession to the Catholic claims. To the historian of the Irish university question these letters are of the greatest importance, but they are not of much interest to the general reader. Possibly they will be used by some student of Irish history who is seeking for a subject for post-graduate research.

So many matters are discussed in the correspondence that it is difficult to select extracts suitable for the space so kindly rendered available by the editor of ECONOMICA. The letters contain numerous interesting references to the events and controversies of the time. Mill was equally at home in the political, philosophical and literary worlds. The American war, female suffrage, parliamentary reform, proportional representation, the affairs of *The Reader* (the brilliant weekly that ran, with many alarms and vicissitudes for nearly four years)—these are among the principal topics mentioned. The student of economics is, however, likely to be less interested in any of these subjects than in the views of the two correspondents on questions of economic theory. The following collection of extracts contains some of the principal references to such questions.

## III

The following two letters deal with the effect of the discoveries of gold on prices which was so hotly debated in the sixties. Henry Fawcett stated at the British Association meeting at Newcastle in August, 1863, that gold had depreciated in value by about ten per cent. and that this depreciation would increase. He thought that the reason why the depreciation had not been greater was that the export of gold to India and China took it out of monetary circulation. *The Times* contained a leading article on September 5th dealing with the subject. According to this article the moderate degree of depreciation in the value of gold had shown that the predictions, and therefore the principles, of the professional economists were fallacious. *The Times* accounted for the moderation in the rise of prices by, first, the absorption of gold in the arts and manufactures and, secondly, the fact that the gold went in the first place to countries of low prices—Continental Europe and the East. " The prices will rise no doubt, but it is elsewhere and to our level that they will rise. Steam and trade have now brought into contact all the nations of the globe, and there is many a thirsty land to be saturated before the rise of the waters will come home to us." On September 9th *The Times* printed a letter from Cairnes challenging these views. Cairnes stated that the reason why the price level had not risen more was that gold was taking the place of silver as a monetary metal in many countries. He strongly controverted the view that the export to areas of low prices (the so-called " hydrostatic " theory) was the operative cause—" This appears to me to be as much opposed to patent facts as to the best established doctrines of economic science ". He expressed the opinion that a further depreciation of gold was unlikely.

CAIRNES TO MILL, DROGHEDA, SEPTEMBER 10TH, 1863: " You have doubtless seen the discussion on gold which was commenced at Newcastle, and is now continued in *The Times*. I confess I have followed it with no small disgust. I do not believe that Fawcett sees into the springs of the movement: a year ago he pronounced that the value of gold had *not* fallen, because, he said, silver had not risen in price: now he says the value of gold *has* fallen, although the price of silver remains just as it was. The flow to the East he points to as a counteracting cause, on the continuance or cessation of which the progress of depreciation is to depend; whereas it is demonstrable—at least it seems so to me—that this is but the *consequence* of the increased supply of the precious metal in Europe—the *mode in which its distribution is effected*. As for the discussion in *The Times*, whether in letters or in leading columns—it seems to me simply discreditable to the state of economic knowledge in the country. A writer the other day laid down the doctrine that gold diffuses itself on hydrostatic principles, and consequently cannot affect English wages or prices till wages and prices in all other parts of the world are brought up to the English level, just as if Ricardo and you had never written, and *The Times* endorses this precious discovery. In still later numbers the absurdities are still more rank. I ventured to interpose a protest in a letter which appeared in Wednesday's *Times*, which possibly you may have seen. Besides the protest, however, it contains nothing that I had not previously said in *The Economist*."

MILL TO CAIRNES, AVIGNON, DECEMBER 26TH, 1863: "Shortly before the gold discoveries, there had begun to take place, and has been taking place ever since, a great increase of facilities of communication and a great enfranchisement of trade; having for their necessary effect to bring the price of many commodities in different parts of the world, far nearer to equality than they had ever been before. Had there been no gold discoveries, this equalisation would have taken place, partly indeed by a rise of prices in the more remote and poorer regions of the earth, but partly also, and perhaps still more, by a fall of prices in the great manufacturing countries. The influx of gold had first to make this phenomenon disappear, before its effect would be apparent at all in a rise of English prices. During the interval it would be steadily raising prices in the distant countries and in those which export raw produce. In those countries the gold would co-operate with other causes tending to a rise; in England it would be acting in opposition to causes tending to a fall; consequently there would be little or no rise of prices in England until there had been a rise in the distant markets sufficient to bring about that nearer approximation of prices in the two regions, which corresponds to the increased facilities of trade. This seems to me to be what the promulgator of the " hydrostatic theory " has in his thoughts: and it would account for the rise of general prices in England being in a considerably less ratio than that of the increased quantity of gold in the world. It certainly appears that the rise of prices has been much more marked and considerable in the places where their range had previously been low; especially those in which hoarding does not prevail, but to some extent even where it does."

## IV

The following group of extracts deals with the preparation of the sixth edition of Mill's *Principles of Political Economy*, which was published in 1865. In the preface to this edition Mill referred to alterations and additions. " The chapter," he states, " in which the greatest addition has been made is that on the Rate of Interest; and for most of the new matter there introduced, as well as for many minor improvements, I am indebted to the suggestions and criticisms of my friend Professor Cairnes, one of the most scientific of living political economists." Cairnes's suggestions for the changes needed in the fifth edition of the *Principles* are contained in a bundle of notes referred to in his letter of November 29th, 1864. In his letter of December 12th, 1864, Mill promised to return these notes. He kept his promise, as the notes were found in Mr. Cairnes's box. They consist of a very long and painstaking series of comments and criticisms on many passages in Mill's fifth edition.

MILL TO CAIRNES, AVIGNON, OCTOBER 3RD, 1864: "I have much work cut out for me to do during this autumn and winter, part of which is that of correcting my Political Economy for a new edition. I should be very glad to make any improvement in it which you can suggest, and especially to know if there is anything which you think it would be useful to say on the present state of Ireland. My speculations on the means of improvement there have been in a state of suspended animation, from which it is almost time that they should emerge."

CAIRNES TO MILL, GALWAY, NOVEMBER 29TH, 1864: "I now send you some ten pages of notes set down in the order which I happened to have them most forward in preparation. You will see that I have, in several instances, made bold to criticise you: for the most part my criticisms do not pass beyond verbal questions; but even when they go no further than this I offer them with the most sincere deference: much more do I feel distrust of my conclusions when I venture to differ from you on points of doctrine. I hope to send you another batch of notes in about a week, the remainder will consist in what I have to say on Ireland and on the theory of money and interest."

MILL TO CAIRNES, AVIGNON, DECEMBER 1ST, 1864: " Have you formed any opinion, or can you refer me to any good authority, respecting the ordinary rate of mercantile and manufacturing profit in the United States? I have hitherto been under the impression that it is much higher than in England because the rate of interest is so. But I have lately been led to doubt the truth of this impression, because it seems inconsistent with known facts respecting wages in America. High profits are compatible with a high reward to the labourers through low prices of necessaries, but they are not compatible with a high cost of labour; and it seems to me that the very high *money* wages of labour in America, the precious metals not being of lower value there than in Europe, indicates a high cost as well as a high remuneration of labour. Supposing profits to be lower than in Europe instead of higher, it is yet quite intelligible that interest might be higher. There is, I apprehend, in America, scarcely any unoccupied class, living on interest: almost everybody is in active business, needing all his own capital and more too. In New England even the banks scarcely have any deposits, the class who in England would be depositors being there shareholders. Consequently the loan market is hardly supplied at all from native sources, except the capital and notes of the banking companies: and when there is a great demand for loans it has to be supplied from the European money market, and therefore at a rate of interest so high as to be a temptation to foreigners. I should be much indebted to you if you could help me on this subject as, if I have been misleading the readers of my Political Economy, it is very desirable that the error should be corrected in this edition.

"I have been obliged to read, with a view to my new edition, the most recent and voluminous of Carey's writings, his *Principles of Social Science*, because his attacks on the Ricardo political economy and on free trade are, some of them if not new, at least made in a new shape, and I have thought it good to give a brief refutation of them the rather as the book is a good deal thought of by some of the French political economists, and is helping to muddle their ideas. The parts of his speculations which I have had to attack are really the best parts, as it was not worth while to notice any errors but those which had some affinity with truths. But it really would be a useful exercise for any clearheaded and painstaking student of political economy to shew up the book, for I think I never met with any modern treatise with such an apparatus of facts and reasonings, in which the facts were so untrustworthy and the interpretations of facts so perverse and absurd. I do not imagine that it would be worth your while any more than mine to take the trouble of reviewing it, but I should very much like to see it properly done. To give a really adequate exposure of the book would be out of the question, for there would be something requiring comment on every page; but a selection might be made, in a moderate compass, which would suffice to destroy any authority the book might have. Withal I cannot dislike the man, for his feelings, and his way of thinking on general subjects, so far as I can perceive, are usually right."

CAIRNES TO MILL, GALWAY, DECEMBER 6TH, 1864: "As to the rate of mercantile and manufacturing profit in the U.S., I have written to a quarter from which I have good hopes of getting information. I have indeed hitherto taken the supposed high rate of profit in U.S. for granted. The high rate of *money* certainly would make one suspect the correctness of this view, but the fact is not conclusive. The precious metals may not be lower *in value* in America than in Europe, but their *cost* is certainly lower; the only question is whether it is so much lower as to render the high rate of profit also higher than, or as high as, in this country. In what you say on the rate of interest in its relation to profit I entirely concur. You will find something on this point in my notes."

MILL TO CAIRNES, AVIGNON, DECEMBER 12TH, 1864: "I do not know how sufficiently to thank you for all you have done for me. That you should have taken the trouble to write out your thoughts so fully on so many points, only for my use, is a favour such as I should never have presumed to ask from you. It is like nothing but the philosophic correspondence in which the thinkers of the 16th and 17th centuries used to compare notes and discuss each other's opinions before or after publication—of which we have so many interesting specimens in the published works of Descartes. I shall keep the notes carefully and return them to you, for I do not like that so much thought, so clearly worked out on paper, should have no reader but me; besides, it enables me with a better conscience to use their contents.

"On most of the minor points I think you are right, and shall profit by your suggestions. On Ireland I shall cancel all I had newly written on that subject, and wait for the further communication you kindly promise. On the few points of doctrine on which our opinions differ, you have not, thus far, convinced me, though you have taught me much. Among these I do not count the theory of the rate of interest, for I agree entirely with your explanation of the phenomena, and the article in the *North British Review* appears to me excellent. I had, even before I heard from you, inserted a passage pointing out how the new gold, as long as it continues to flow in, must tend to keep down the rate of interest. We differ, I believe, only on a question of nomenclature, and at present it seems to me that the objections to your phraseology are stronger than to mine. But I have not done thinking on the subject, and I shall in any case have to modify several expressions, if nothing more.

"In the matter of the operation of duties on international values, I see that I have omitted one of the elements of the question, viz.: the competing demands of other commodities on the purse of the consumer; but it does not seem to me that this omission materially affects the conclusion. Suppose that I have a given sum, say £10 a year, the expenditure of which I am determined whatever happens to divide between two commodities, *A* and *B*. I conceive that even then, if *A* rises in price and *B* falls, the effect in the average of cases will be that I shall buy more of *B* and less of *A*.

"On the Wakefield system I scarcely understand your argument. In the supposed case of the settlers, and in every other, I apprehend the separation of employments to be a real cause and indispensable condition of a larger production. It is true that territorial separation of employments, by international trade, often suffices; but the main justification of Wakefield's system is, that this trade does not take effect when families settle, each of them many miles from its next neighbour in the wilderness.

"The point on which we seem to differ most, and to be least likely to come to an agreement, is the income tax. You think it fair to take from different people in a single year, an equal percentage of what their incomes, whether permanent or temporary, would sell for in that year: because (you say) the payment in each year should be compared with what the income is worth in that year to its owner. In this I agree; but I answer, that the income is, in that year, worth *to him* its capitalised value only on the supposition that he actually capitalises it, and spends the whole value within the year. Then, indeed, he will have been fairly taxed: but then, he will not have to pay the tax in any future year, for the income will have passed into other hands. On any other supposition the income is only worth to him its capitalised value spread over the whole of its duration, that is, in each year the total amount divided by the number of years. I agree in what you say about equality of sacrifice, but in estimating this I only exclude necessaries. I do not think a distinction can be fairly made between comforts and luxuries, or that I am entitled to call my tea and coffee by the one name, and another person's melons and champagne by the other. I allow for nothing but what is needed to keep an average person alive and free from physical suffering."

MILL TO CAIRNES, AVIGNON, JANUARY 5TH, 1865: "Of the two or three points which we differ about, I will only touch upon one—the influence of price on demand. You say, if a tax is taken off beer and laid on tobacco in such a manner that the consumer can still, at the same total cost as before, purchase his usual quantity of both, his tastes being supposed unaltered, he will do so. Does not this assume that his taste for each is a fixed quantity? or at all events that his comparative desire for the two is not affected by their comparative prices? But I apprehend the case to be otherwise. Very often the consumer cannot afford to have as much as he would like of either: and if so, the ratio in which he will share his demand between the two may depend very much on their price. If beer grows cheaper and tobacco dearer, he will be able to increase his beer more, by a smaller sacrifice of his tobacco, than he could have done at the previous prices: and in such circumstances it is surely probable that some will do so. His apportionment of self-denial between his two tastes is likely to be modified, when the obstacle that confined them is in the one case brought nearer, in the other thrown farther off."

CAIRNES TO MILL, DUBLIN, JANUARY 9TH, 1865: "Touching the taxation question, after weighing carefully what you say I am still inclined to think that the position is substantially sound that "a man's comparative desire for two commodities is not affected by their comparative prices". The animal propensity towards beer and tobacco in certain proportion to each other depends on physical conditions: I can conceive that these may be overborne in some degree by the force of mental impressions; but then I think the mental impressions depending for their force on the principle of association are liable to become weak, while the force of the former is a constant quantity. At all events we have, I think, brought the question to a point at which it can only be decided by experiment, which, next to agreement, is the most satisfactory issue of an economic argument . . .

". . . I intended in a former letter to have suggested to you the advisability of adding an index to the new edition. I often myself feel the want of one."

MILL TO CAIRNES, BLACKHEATH, FEBRUARY 4TH, 1865: "The Political Economy has gone to press, considerably improved as I think, and indebted to you for much of the improvement. . . The subject of an Index I had thought of but most Indexes of philosophical treatises are so badly and stupidly done, that unless I could have made it myself or got it made by a political economist, I thought it better let alone: An index is less wanted for a systematic treatise than for a book of a miscellaneous character, as the general arrangement of topics, added by the analytical table of contents, shews where to find the things most likely to be wanted."

MILL TO CAIRNES, BLACKHEATH, MARCH 5TH, 1865: "The American information is very valuable and I can hardly be thankful enough to Mr. Ashworth and to his Boston correspondent for the trouble they have taken and the service they have done me. I beg you will convey to Mr. Ashworth my grateful acknowledgments. From their statements it is clear that the ordinary notion of the extravagantly high rate of profit in the United States is an exaggeration, and there seems some doubt whether the rate is at all higher than in England. But that does not resolve the puzzle, as even equality of profits, in the face of higher cost of labour, indicated by higher money wages, is as paradoxical as superiority. This is the scientific difficulty I mentioned, and I cannot yet see my way through it."

CAIRNES TO MILL, GALWAY, MARCH 13TH, 1865: "I see my observations on American wages and profits in their connexion with the theory of profit did not hit the mark; and I fear I must now relinquish the hope—I might say the ambition—of doing this, as on the assumption that the exposition I gave was correct—which you concede to me—I am unable to perceive where the difficulty lies: in short, the scientific problem seems to me to be solved. For the rest, it is (to my apprehension) merely a matter of evidence whether money wages and profits *are*, at one and the same time, so high as is alleged: if they *are*—then the fact on the assumption that my exposition was correct is conclusive, as it seems to me, that the difference between the cost of obtaining the precious metals in America and in England *is* great enough to produce the results which we see. Am I guilty of arrogance in suspecting that the difference between us here—my inability to perceive the difficulty of which you are sensible—is due to the greater simplicity of the theory of profit through which I look at the phenomena?—I refer to that mode of stating the doctrine—differing from yours and Ricardo's only in form—of which a sketch was contained in the papers I sent you. Of course if the theory, thus stated, failed to embrace any essential condition, this would be simply its condemnation; but it appears to me to embrace all the conditions included in your doctrine of "cost of labour", and it renders the phenomena in the case with which we are now concerned unless I deceive myself perfectly intelligible. Might I ask as a favour, when you come to deal with this question at your leisure, that you would consider once again that mode of stating the theory."

MILL TO CAIRNES, BLACKHEATH, MARCH 22ND, 1865: "I have again gone through your exposition of profits in the papers you so kindly took the trouble of writing for me; and I think, as before, that your mode of putting the doctrine is very good as one among others, and that there is no difference of opinion between us. I still, however, prefer my own mode of statement, for reasons which it would be long to state, and which I have not time at present to reconsider from the foundations. I am inclined to think that the real solution of the difficulty, and the only one it admits of, has been given by myself in a subsequent place, Book III, ch. xix, §2 (vol. ii, p. 156, of the fifth edition)."

## V

The following letters arose out of Mill's well-known review of Thornton's "Labour and its Claims" in the *Fortnightly Review* of May, 1869. In this review Mill, while admitting Thornton's main position in regard to the wages fund, challenged his argument regarding the effect of demand and supply on market prices. Thornton gave by way of example a number of exceptional cases which can all be analysed as imperfect competition leading to exploitation of one bargaining party by another. For example, sellers with strong reserves can withdraw part of their stock and thereby raise the price of what they sell. Thornton pointed out that in the labour contract the buyers normally have strong reserves and can therefore influence the price of what they buy. Cairnes returned to this matter in his *Leading Principles of Political Economy* (p. 101) where he stated that "Mr. Mill's doctrine of market price has been challenged by Mr. Thornton, and in my opinion successfully; but I prefer to state my objection to it in my own way".

CAIRNES TO MILL, AIX LES BAINS, MAY 23RD, 1869: "I received the *Fortnightly Review* for May some days ago, and, as you may suppose, was not long in devouring your article on Thornton. I am delighted with it both on his account—for it can hardly fail to send every political economist to his book—and also for its own sake. All that you have said on the subject of the wages-fund seems to me excellent. The conception, as now delineated, is, so far as I can see, invulnerable; while it retains all that is required to serve as a basis for a theory of wages. In your remarks on the bearing of Thornton's theoretical views on the practical portion of his book, you seem to me to have supplied a *nexus* that was needed; and I trust he will avail himself of the suggestion in subsequent editions. I am obliged, however, to confess that, in reading the earlier portion of the article—that in which the demand and supply theory is discussed—I experienced the unusual sensation of being unconvinced by your reasoning. I cannot easily describe my distrust of myself and the discomfort I feel when I find myself in the position of challenging your arguments: the occurrence, however, brings me this one compensation, that it gives me, in some sort, a guarantee of my independence in the innumerable instances in which I acquiesce in them. I shall now endeavour to state in as few words as I can the grounds on which I venture to dissent from your teaching in the present instance.

"Accepting the doctrine that cost of production, in the case of all freely produced commodities, regulates average value, and that this regulation is effected through what is loosely called the play of demand and supply, the problem is to state the conditions which govern temporary values—i.e., the actual prices at which commodities are sold from moment to moment; and the solution for which you contend is that price at any given moment is determined by the condition that it shall be such as shall render the quantity demanded and the quantity supplied equal at that moment. Well, I admit at once that in the sense partly defined, and partly I think implied, in your remarks at pp. 512–13, the doctrine is true; the quantity demanded and the quantity supplied are evidently, in the sense there indicated, *always* equal at the moment of sale (for it seems to me that your qualifications cover all Thornton's hypothetical cases). But then I contend that the doctrine thus qualified amounts to an identical proposition, while I cannot see that it throws any light on that for the elucidation of which alone a theory in the case is needed—the conditions which [?] temporary prices. The qualification to which I refer is contained in the statement—Reserving a price is to all intents and purposes withdrawing supply.

"In order to bring out my meaning, I will put a case slightly different from that on which you have argued. A farmer, I will suppose, has 100 sheep to sell, and takes them to market. Having previously used such means of information as were open to him, and having seen the supply and buyers in the market, he finally takes his decision that, while endeavouring to get as much more as he can, he will not let his sheep go for less than 40/– a piece. On proceeding to sell he is only able to find purchasers at this price for 80, and consequently he takes home 20 sheep unsold. Here, as I understand the qualification quoted above, the 20 sheep taken back, although offered on the same terms as others which are sold, are not to be counted as supply. They may have been offered for sale, but they have been withdrawn. Now it seems to me that this amounts to making *demand the measure of supply:* nothing being counted as supply but that for which a demand can be found. And though you have not stated it, I think it is evident that demand must be understood with a similar qualification. Thus, taking the converse of the above supposition, a farmer, coming to buy sheep might decide to take 100 at 40/– if he could get them, but not to give more; and then, in the event of his only finding 80 for sale at this price, he would return home with 20 short of the number he really desired to buy. Here, to make the doctrine true, the demand must be considered as being for 80 sheep only, not for 100—that is to say the *supply is, in its turn, used to measure demand.* The doctrine is thus, if I have correctly apprehended it, only saved by being reduced to an identical proposition. The question, of course, remains as to how far the above illustrations are typical: it seems to me that they are fairly so. The former is the case of all dealers who keep a stock of goods on hand of which they are always ready to sell as much as they find a demand for at what they consider a remunerative price: both cases, I think, represent the common practice of the wholesale markets.

"But, further, I am unable to see that the doctrine throws any light on the conditions which really determine temporary prices: indeed it seems to me that it even diverts attention from those conditions. For example, in your illustration of the sheep at p. 512, you say: When no more than 40/– a head can be obtained for sheep, all sheep whose owners are determined not to sell them for less than 50/– are out of the market, and *form no part at all of the supply which is now determining price.* But is this certain? Is it not possible that the presence of the sheep in question in the market may have an influence in determining the price realised in other cases? It is surely not an improbable supposition that a purchaser, on entering a market, should take account as one of the conditions of the practical question he has to solve—of the whole supply which he sees before him, and that this should be an element in guiding his mind to the final decision on which he acts. If so, then, those sheep, present in the market, but which are not ultimately sold, do nevertheless form a part of the supply which determines price in that market. But I go further than this, and add that the imaginary purchaser will not improbably take into his calculation the existence of supplies, not in the market at all, but which he knows to be in the country; or even (if we substitute commodities in general for sheep) of supplies which, though not in the country, he knows to be procurable from other countries. Nay, he will sometimes even go further still, and consider the supplies which may be forthcoming when productive operations now going forward shall be completed. He will, moreover, consider many other things besides supply—such as the facilities for maintaining stock (in the case of animals the abundance or scarcity of pasture at the moment) the perishableness or otherwise of the commodity dealt in, and many other things which will easily suggest themselves. These and similar circumstances, it seems to me, are not less than the commodities actually sold, real conditions determining the fluctuations of prices from month to month and from day to day; for they form equally with them, the data—or, to put the point more distinctly—they are the constituent elements of the opinions of vendors and purchasers, which opinions, coming into contact in the higgling of the market, are the proximate causes of market price. I hold, therefore, that a theory of which the object is to throw light on the phenomena of temporary prices, is bound to embody and estimate those conditions, or, at least as many of them as are capable of being set forth in distinct terms. But the doctrine of demand and supply, as expounded in the essay, not only does not embody them, but pointedly excludes them from consideration. For this reason it seems to me to fail in the true end of a theory—that of explaining the phenomena.

" I must own at once that I am not myself prepared to produce a formula that will satisfy the requirements I have indicated above; nor do I see that anything is gained by adopting Thornton's suggestion that " competition " should be substituted for " demand and supply ". The elements of the problem appear to me to be too numerous and too indeterminate to admit of a generalisation which should accurately embody them all. But I think something might be done towards clearing up the facts by a classification of the more important of the various influences (such as I have indicated above) which weigh with dealers—whether vendors or purchasers—in coming to a decision on price, accompanied with an indication of the greater or less applicability of each in the case of leading commodities. After all, however, it may be doubted if the game would be worth the candle. So far, at least, as commodities are concerned, I do not see that a theory of *market* (as distinct from average) prices is of very much moment in a scientific point of view; and I should, for my part, be content with indicating in a general way the *kind* of causes which determine them. The case is different, however, with wages, the permanent causes controlling which operate too slowly to satisfy the requirements of social reform. Fortunately it happens in this case that the conditions affecting market prices are far less complex than occur in the case of commodities, and the conception of the wages-fund, explained as you have now explained it, taken in connexion with the special circumstances under which labour is offered in the market, which Thornton has done so much to elucidate, supply together, it seems to me, data from which an adequate statement of the economic law of wages might without much difficulty be evolved."

MILL TO CAIRNES, AVIGNON, JUNE 23RD, 1869:[1] " I have had so much to do, and so many other letters to write, that I have delayed till now thanking you for your most acceptable letter of May 23rd, and especially for the sifting which you have given to my review of Thornton. You may imagine how gratifying it is to me that you give so complete an adhesion to the view I take of the wages fund. In regard to the general subject of demand and supply, I think there is not, at bottom, any considerable difference between us. My object in the *Fortnightly* was to shew that the cases supposed by Thornton do not contradict and invalidate, as he thinks they do, the equation of supply and demand. In this you agree with me, and you do not think the doctrine incorrect. The amount of its value, either scientific or practical, is a different question. But, while I admit almost all that you say, I think that the proposition as laid down is something more than an identical proposition. It does not define nor did it, as I stated it, affect to define the causes of variations in value. But it declares the *condition* of all such variations, and the necessary *modus operandi* of their causes, viz.: that they operate by moving the supply to equality with the demand, or the demand to equality with the supply. The numerous considerations which you notice as influencing the minds of sellers are, all of them, considerations of probable future demand and supply, modifying the effect which would take place if nothing but present facts were considered. Now it appears to me important to point out that these prospective considerations operate by inducing the sellers either to convert a possible present supply into an actual one, or to withdraw an actual present supply into the region of merely possible ones; and that in either case the relation of the price to the actual supply and demand is constant, i.e., the price is that which will make them equal. If this statement does no more than give a distinct scientific expression to what is already implied in the terms used, still it is not unimportant to evolve and make explicit what the facts of purchase and sale and a market price really involve."

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[1] This letter is included in Elliot's collection, Vol. II, p. 207.

## VI

The last group of letters from which some extracts have been chosen deals with the theory of value. The discussion opened in a letter addressed by Mill on May 15th, 1872, which is printed in Elliot's collection (vol. II, p. 340). Mill requested Cairnes to let him see what he was writing on certain points in the theory of value.

CAIRNES TO MILL, LONDON, JUNE 16TH, 1872: "I shall be exceedingly pleased to let you see what I have written on the theory of value, so soon as it is in a fit condition to be submitted to you. I am inclined to maintain that the question of cost of production is more than verbal, though this may seem scarcely reconcilable with what I freely admit, that I start from your fundamental positions, and am brought, in the main, to conclusions identical with yours. But my point is—how far I have succeeded in making it out you will be able to judge when you have seen my papers—that your doctrine of cost of production as consisting of wages and profits has no logical connection with your fundamental assumptions, and will not yield, in fact is inconsistent with, your conclusions on the subject of value. For example, I think it may be shown, that, regarding cost of production as consisting of wages and profits, all values, international values among the rest, are in proportion to cost of production. You have not been led to this conclusion as regards international values, because in investigating them you have fallen back on what I regard as the true idea of cost, as consisting of labour and abstinence. You will see from this that the effect of my argument, supposing I am able to make it good, will not be to disturb either the established foundations or conclusions of Political Economy, but to bring them into closer and more direct connection, and, as I hope, to give increased consistency and coherency to the general doctrine."[1]

MILL TO CAIRNES, AVIGNON, OCTOBER 4TH, 1872: "Your discussion of the question whether wages ought in any sense to be considered as cost of production, or whether that term should be exclusively predicated of labour and abstinence, was always likely to be scientifically instructive, but I now perceive that it will have a special value *de circonstance*. You must have been struck, as I have been, by the thoroughly confused and erroneous ideas respecting the relation of wages to price, which have shewn themselves to be almost universal in the discussions about the recent strikes. The notion that a general rise of wages must produce a general rise of prices, is preached universally not only by the newspapers but by political economists, as a certain and admitted economical truth; and political economy has to bear the responsibility of a self-contradictory absurdity which it is one of the achievements of political economy to have exploded. It provokes one to see such ignorance of political economy in the whole body of its self-selected teachers. *The Times* joins in the chorus, notwithstanding Mr. Courtney, who, I do hope, has no hand in the matter. Certainly no one who knows, even imperfectly, what the Ricardo political economy is, whether he agrees with it or not, can suppose this to be it. I hope you will come down upon it with all the weight of your clear scientific intellect, your remarkable power of exposition, and the authority of your name as a political economist."

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[1] Mill's reply to this letter, dated August 2nd, 1872, is printed in Elliot's collection, Vol. II, p. 343. In this letter Mill said: "I think it very improbable that on a question of abstract political economy we should not agree".

# A Note on Speculation and Income Stability

By S. C. Tsiang

## I

In the October, 1939, issue of the *Review of Economic Studies* Mr. Kaldor has attempted to demonstrate in a brilliant article entitled " Speculation and Economic Stability " that the instability of our economic system can be attributed to speculation in stocks of commodities and particularly in stocks of securities. It is stated that when the current demand for any commodity, or for securities (in which case the current demand constitutes the current savings in the orthodox sense), increases and the speculators, by selling existing stocks of commodity or securities, prevent the relevant price from rising to the necessary extent to restore the balance between the current demand and supply, the level of income will decline until the flows of current demand and supply are once more equated by the decline of income. Conversely, if when the current demand falls, or the current supply increases, the speculators, by buying the current supply to add to their stocks, prevent the price from falling to the necessary extent so as to balance the current supply and demand, the level of income will tend to rise until the equilibrium between the flows of current supply and demand is restored again by the rise in the level of income. For, to quote Mr. Kaldor, " an increase in speculative stocks of any commodity implies an increase of investment in that commodity, or to use Mr. Hawtrey's expression, a release of cash by the market. (Conversely a reduction of stocks implies an absorption of cash). Unless this increase is simultaneously compensated by a decrease in the amount of stocks held in other commodities, it must imply a corresponding change in the level of investment for the system as a whole ".[1]

Economists who have been incorrigibly inculcated with the orthodox way of thinking will, however, be prompted to go on to ask, will the reduction of speculative stocks necessarily imply an absorption of cash, or conversely an increase in speculative stocks a release of cash ? In other words, will the speculators who have sold part of their stocks retain the proceeds in idle cash, or will not they invest them almost immediately in other assets? If they buy and add to their stocks, will they have a large balance of idle cash and release cash therefrom as they buy ? If Mr. Kaldor had followed Lord Keynes in assuming that the speculators in the security market generally hold part of their wealth in the form of a speculative balance of idle cash which, though bringing in no monetary return, yet yields the same satisfaction at the margin as other income-yielding assets in the form of safety and convenience, then everything would be easy to understand. If an increase in the flow of current savings to the security market tends to depress the rate of interest (i.e., to raise the prices of bonds), the speculators, who somehow expect that the high prices will not last and anticipate them to fall back again thus involving them in a capital loss, will then substitute cash for securities. The cash embodying the flow of current savings offered to the security market will be partly absorbed into the speculative balances of the speculators. In the case where the stocks of ordinary commodities are the object of speculative selling, there is no reason to expect that the speculators will add the entire proceeds to their idle balances; for the interest rate which is supposed to determine the size of speculative balance will not be directly affected. If, however, the proceeds are transferred to the loan market and tend to lower the rate of interest, then again part of the cash proceeds will be absorbed into the speculative balances.

Mr. Kaldor, however, has explicitly rejected the idea that in normal circumstances the speculators hold large balances of idle cash and substitute cash for securities or vice versa as they speculate on the long-term capital market. For his theory of speculation in the capital market is based on the recent theory of the relationship between the long-term and short-term rates of interest as developed by Mr. Kalecki, Professor Hicks and Mr. Kaldor himself. According to them the current long rate must equal the average of the current short rate and the expected future short rates over the period of currency of the long-term bond concerned, plus or minus a certain premium to allow for the speculator's risk due to uncertainty of their expectations according as they are buyers or sellers of the stock of bonds.[1] If the current demand for investment " finance funds " and the current supply of loanable funds at the long-term capital market tend to make the current long rate diverge from the average of the current short rate and the speculators' expected future short rates discounted for uncertainty (discounted positively when the speculators intend to sell long-term securities, discounted negatively, i.e., risk premium will be added to the expected future rates, if they intend to buy additional stocks of bonds), the speculators will think it profitable to substitute short term bills for long-term bonds or vice versa, even though the present short rate may have fallen or risen relatively to current long rate. The kind of speculation in the capital market as envisaged by Mr. Kaldor is therefore essentially an arbitrage operation between the short-term money market and the long-term capital market. Short-term bills which include savings deposits, call loans, etc., on the other hand are rightly considered as not subject to speculation, for the lifetime of short-term bills is too short for the capital values of bills to be appreciably affected by a change in short rate and big changes in the short rate are not expected in such short period.[2] It is only when the short-term rate of interest is lowered to such extent as not even to compensate the negligible risk attached to short-term assets and the inconvenience as compared with cash that idle cash will be held instead of short-term assets.[3] Therefore, speculation on the long-term capital market means substitutions between long-term securities and short-term bills. To quote Mr. Kaldor's own words, " there is no reason to expect, in normal circumstances at any rate, that the substitution (when speculators sell bonds) will be in favour of cash. ' Idle balances '—i.e., that part of short-term holdings which the owner does not require for transaction purposes—can be kept in forms such as savings deposits, which offer the same advantages as cash (as far as the preservation of capital value is concerned) and yield a return in addition."[4]

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[1] Kaldor, *loc. cit.*, p. 17.

Now if the speculators in the stock exchange do not in normal circumstances keep idle speculative balances of cash, it is obvious that the stock market itself does not absorb or release cash, unless we contend that larger turnover on the stock exchange may tie up more cash there for transaction purposes. This latter contention, however, is of minor practical importance, for it is well known that the turnover on the stock exchange is chiefly effected by clearing arrangements and very little cash is required,[5] and it is certainly not what Mr. Kaldor meant by the absorption or release of cash due to speculation. What happens when speculators on the stock exchange sell their stocks of old securities is that they will immediately transfer the proceeds to the short-term money market and invest there in short-term assets. If the cash proceeds are to be absorbed they must be absorbed in the last resort by the short-term money market.

This argument on somewhat *a priori* grounds is perfectly in accord with the findings of Professor Machlup's penetrating and realistic investigation of the actual working of the stock market.[6] There Professor Machlup has shown that when the speculators sell their stocks of old securities thus absorbing part of the inflow of liquid funds into the capital market, they will generally use the proceeds either to pay off their margin debts with their brokers, or, if they have no margin debt to pay off, simply leave them on their brokerage deposits with the brokers, or they may withdraw the proceeds and lend at call on the money market. Call loans are generally regarded as just as liquid as cash and yield in addition a profitable return. If they leave the proceeds with the brokers or repay their margin debts, the brokers will not keep the money in idle cash either. For the transactions on the stock exchange are effected entirely by clearings and consequently there is little need for the brokers to keep a large cash balance. In practice the brokers generally keep a very small fraction of their customers' brokerage deposits with them in balances of cash (i.e., demand deposits with the banks) and even a smaller fraction of their turnover. Once their cash balances exceed the minimum, they will immediately use them to repay their vast amount of outstanding debts or to a much smaller extent to grant short loans. In either case, that part of the inflow of the liquid funds into the capital market absorbed by the speculative sellers is transferred to the short-term money market.[1] Similarly when the flows of the demand for loanable funds for industrial purposes and the supply of loanable funds out of disposable income tend to raise the long-term rate beyond what the speculators believe to be the safe level, the speculators who buy additional securities to increase their holdings of stocks will not have the idle cash balances to release. What happens in practice is that the bullish speculators will give orders to their brokers to buy additional stocks with their brokerage deposits at their brokers, or, if they have exhausted their brokerage deposits, by incurring new "margin debts" with their brokers. The brokers, who, as we have observed, do not as a rule keep more than a minimum fraction of their customers' brokerage deposits with them in balances of demand deposits, will have to pay the sellers of securities, who wish to withdraw the monies to finance real investments, by borrowing new loans from the banks or other lenders in the money market, or by calling in their call loans. Thus the excess of the outflow of loanable funds over the inflow of savings on the long-term capital market must be raised by the speculators or their brokers from the short-term money market and transferred to the long-term market. In other words, part of the current demand for loanable funds on the long-term market is transferred by the speculators to the short-term market.

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[1] Mr. Kaldor originally held the view that the current long rate must equal the average of the expected future short rates plus a positive premium for risk due to uncertainty. However, in view of his later opinion in a symposium on the theory of the forward market, the above modified version will probably be his view now. (Cf. N. Kaldor, " A Note on The Theory of Forward Market," *Review of Economic Studies*, Vol. VII, pp. 196–201.)

[2] N. Kaldor, *Speculation and Economic Stability*, p. 13, footnote 2.

[3] *Ibid.*, p. 14, footnote 1.

[4] *Ibid.*, p. 13, footnote 2.

[5] Cf. Keynes, *A Treatise on Money*, Vol. I, p. 249, 2nd paragraph. Also F. Machlup, *Stock Market Credit and Capital Formation*, Chap. VI.

[6] Machlup, *The Stock Market Credit and Capital Formation*, Chaps. VI, VII and VIII.

The function of speculation in securities is therefore merely to arbitrage between the long-term capital market and the money market. This arbitrage operation by itself certainly does not necessarily bring about instability in money income. It may disturb the equilibrium between the inflow and outflow of liquid funds on the long-term capital market, but if it sets up an opposite disequilibrium between the inflow and outflow on the money market to the same extent, there need not be any change in the stream of money expenditure. Similarly when speculators in other commodity markets sell their stocks, they will normally likewise transfer the proceeds to the money market and invest them for the time being in the more liquid types of short-term assets, e.g., call loans. And when the speculators add to their stocks they will have to raise the cash from the money market either by calling in their loans or by fresh borrowing.[2] It is therefore clear that speculation on the stock exchange, in bonds as well as in shares, or indeed in any other commodity market, and the variations in the size of speculative stocks do not provide the whole explanation of the instability of money income. Speculation in the stock exchange merely transfers, so to speak, the whole burden of adjusting savings and investment to the money market. It is the money market which is responsible in the last resort for the release or absorption of cash and which is, therefore, in that sense the source of instability.

That Mr. Kaldor should have overlooked that the ultimate mechanism which produces instability of money income is the short-term money market is, I believe, largely because of the method of approach he has adopted in dealing with the short-term rate of interest. Although he successfully combines a loanable fund approach (a flow analysis) with an analysis of speculation in stocks with regard to the long-term capital market, he reverts abruptly to a supply and demand for money approach and pushes the flows of loanable funds entirely into the background in respect of the short-term money market. According to him, the short-term rate "is not dependent on expectation at all (or only to a very minor extent) but only on the current demand for cash balances (for transaction purposes) and the current supply". And he depicts the nature of the equilibrium in the short-term interest market by the accompanying

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[1] It is very interesting to note that this is in fact Lord Keynes's opinion in his *Treatise*. For according to that book, the bears will either put the proceeds of their sales on the savings deposits, or else use the "bear funds" to buy Treasury Bills and to grant loans to the money market and the stock exchange. (See Keynes, *A Treatise on Money*, p. 252.)

[2] Speculation in foreign exchange may constitute an exceptional case, if the "speculator", or rather the price-stabiliser, is the monetary authority or an exchange equalisation fund which does keep large funds of idle cash. In such case when they sell foreign exchanges, cash is directly absorbed or cancelled, and when they buy foreign exchanges cash is directly released without passing through the mechanism of money market.

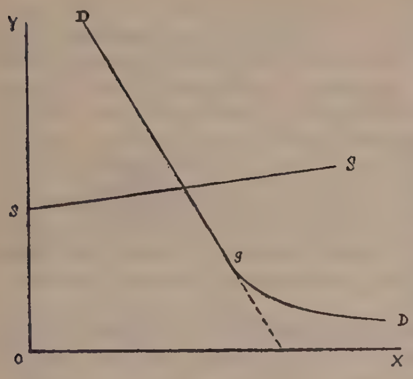

Fig. 1

diagram, where the quantity of cash (i.e., bank notes and demand deposits; savings deposits are excluded, being regarded as a kind of short-term bill) is measured along *OX*, the short-term interest rate along *OY*. "*DD* and *SS* stand for the demand and supply of money respectively. The demand curve is drawn on the assumption that the volume of money transaction (i.e., the level of income) is given. This demand curve is inelastic since the marginal yield of money declines rapidly with an increase in the proportion of the stock to turnover. Below a certain point (*g*) the demand curve becomes elastic, however, since the holding of short-term assets other than money is always connected with some risk (and perhaps inconvenience), and individuals will not invest short-term if the short-term rate is lower than the necessary compensation for this ".[1] The short-term interest rate is determined by the intersection of these two curves.

Although it has been repeatedly demonstrated by Professor Hicks that the loanable fund approach and the demand and supply of money approach are but two alternative ways of saying the same thing,[2] the above procedure is misleading, however, in one respect. For it will be seen that there is no unique and definite demand schedule for money, even if we start from a given level of income. The demand for cash is in fact neither independent of the inducement to invest nor of the desire to save. For an increase in the desire to borrow for investment implies an increase in demand for cash as investment finance fund in the first instance, and an increase in the desire to save involves an increase in the willingness to part with cash out of disposable income.[3] The demand schedule for money is therefore not an independent variable even with a given level of income. It will be shifted about by changes in investment borrowings and in savings before the level of income has as yet changed. To assume that a definite demand curve for money can be drawn, given the level of income, is therefore strictly speaking illegitimate and may prove rather misleading.[4] This will become amply evident when we return to our problem in hand. For if we follow Mr. Kaldor in assuming that the demand curve for cash is given, given the level of income, we shall be unable to see the immediate repercussion of speculation in the long-term capital market, or in any other commodity market, on the short-term money market. Indeed this kind of approach leads Mr. Kaldor to say that " a change in the long-term rate (either the current or the expected rate) cannot react back on the short-term rate except perhaps indirectly by causing a change in the level of income and hence in the demand for cash " (*loc. cit.*, p. 13, footnote 2). This is nothing but a restatement of the Keynesian doctrine that saving and investment have no direct effect on the rate of interest except indirectly through the level of income. Only it is now supposed to have a limited application to the short-term rate of interest only. This, however, is also untenable. For, as we have observed above, the selling of old securities by the speculators in the long term capital market implies that part of the flow of cash offered to the capital market will be transferred to the short-term money market; and this certainly will have a direct effect on the equilibrium of the short-term money market. In other words, the demand curve for cash will be displaced bodily to the left because of the speculative selling of old securities on the stock exchange, although the income level has not as yet fallen to a new level. Indeed the level of income need not fall, if the cash or liquid funds transferred by the speculators to the money market get invested in real goods without appreciable delay through the mechanism of the short-term rate of interest.

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[1] *Loc. cit.*, p. 14, footnote 1.

[2] J. R. Hicks, " Mr. Keynes' General Theory of Employment," *Economic Journal*, June, 1936. Also Hicks, *Value and Capital*, Chap. XII.

[3] That any borrowing for investment implies a preliminary demand for finance funds and thus must have a direct influence on the rate of interest before any change in income has yet occurred is admitted by Lord Keynes. (Cf. Keynes, " The Alternative Theories of the Rate of Interest," *Economic Journal*, 1937.) It is, however, not yet generally recognised that a change in the propensity to save would likewise involve a change in the demand for money, although it is quite obvious with Professor Robertson's terminology. An increase in the propensity to save implies a decrease in the public's planned consumption in the coming period. Less cash will therefore be kept to finance consumption, and more cash out of disposable incomes, i.e., incomes of the preceding period received in cash, will be offered to the security and money markets.

[4] This point perhaps deserves a little more emphasis than a mere *obiter dictum* in this note. For it reveals the logical weakness of the Keynesian doctrine which treats the demand for money as an independent function of the rate of interest and the level of income, independent of the marginal efficiency of investment and the propensity to save; whereas the two latter functions are supposed to affect the rate of interest only indirectly through their effect on the income level. The fact, however, is that both these functions have a direct effect on the demand for money and so on the rate of interest. It should also be noted that Professors Hicks' and Lange's attempts to reconcile the Keynesian theory with the classical theory appeared to have overlooked this point, in so far as they regarded the liquidity preference function (in its broad sense) as independent of the savings and investment functions. This assumption inevitably led them to the Keynesian conclusion that savings and investment affect the rate of interest only indirectly through their effect on the level of income. (See Hicks, " Mr. Keynes and the classics," *Econometrica*, 1937, and Lange, " The Rate of Interest and The Optimum Propensity to Consume ", ECONOMICA, 1938.)

How the mechanism of money market actually works and why it should fail to adjust the inflow of funds to the outflow can only be clearly observed with the help of a loanable fund approach of the Robertsonian type. We shall therefore attempt to describe the nature of the determination of the short-term interest rate in terms of loanable funds in order to bring out in strong relief the impact of speculation on the money market. In order to make the task of exposition at all manageable, we shall make certain simplifying assumptions. We assume after Professor Robertson that there is a period of time, a " day ", which is so short that all active money is exchanged against commodities and services (excluding securities) only once during the day, and the money received in payment for commodities and services on a given day cannot be allocated to any further use during the course of the same day.[1] In other words, the transaction velocity of active money is one in a day; the transaction velocity of active money being assumed to be the same everywhere, so as to avoid the difficulties of averaging and of deciding which piece of money is active or idle, and also to avoid the complication that changes in the disposition of money receipts between the different lines of expenditure may involve changes in the average transaction velocity of money. Money stock (i.e., cash) which is not required to finance the expenditure during the day will be invested on short-term assets (e.g., savings deposits, Treasury Bills, Call Loans, etc.) unless the short-term rate of interest has fallen so low that it does not even compensate the risk and trouble involved. The transaction velocity of active money may certainly change over time due to changes in business confidence and in the usage of money substitutes, such as book credits and commercial bills, but in the short period such as the " day " it is assumed to be independent of the influence of the rate of interest. Financial transactions, viz., the exchange of money against securities and the transference of funds from the long-term capital market to the money market, however, are assumed to take no appreciable time.

The nature of the equilibrium of the short-term money market can then be shown by the accompanying diagram. Let *DD* be the demand to borrow, at the money market, liquid funds (i.e., money) which the industry will actually expend during the same day. Let *SS* represent the supply of liquid funds (embodied in money of course) to the money market on the same day. It consists of that part of the gross savings which is offered to the money market out of the money receipts of the previous day which become disposable only to-day (including therefore depreciation allowances and liquidated capital). To this we must add the possible relending of the liquid funds raised from the long-term capital market, for people who have raised all the funds necessary to finance their whole investment projects, the construction periods of which may spread over a number of days, will find it profitable to retain only that part of the liquid funds that is necessary to finance their expenditures to-day and to relend the rest to the money market only to withdraw therefrom day by day the funds that will be needed for the expenditure of each future day. When the borrowing on the long-term capital market is equal to the gross savings that have flowed into the capital market and the demand and supply for short-term funds at the money market as defined above are also equal, then the stream of money expenditure is at equilibrium, without tendency either to rise or to decline. When the short rate falls very low, there will be further demand for liquid funds which people will like to hold as idle balances in preference to short-term securities. This will make the demand

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[1] I am inclined to think that it is more convenient to take as the minimum period of lag the circulation period of money against the total commodity turnover than the circulation period of money against real income.

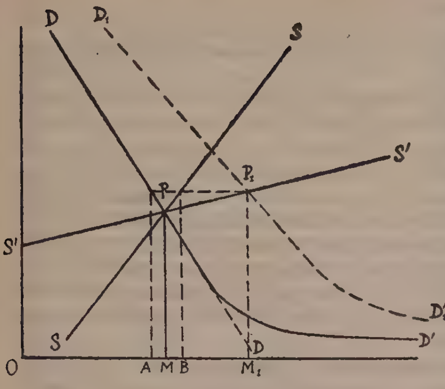

Fig. 2

curve for liquid funds very elastic and asymptotic to the *X* axis at its lower end. We have so far excluded from our *SS* curve the supply of additional bank money. In fact, however, the banks can always supplement the inflow of liquid funds into the money market by creating new bank credits or reduce the supply of liquid funds by cancelling part of the cash flowing into the money market; a fact which has been recognised by economists, e.g., Thornton, Ricardo, and Joplin, since the times of the Bullionist and the Currency Controversies. The supply of bank money has a dominant influence on the short-term rate of interest because of its great elasticity except at the top of boom.[1] Taking the potential expansion and contraction of bank credit into consideration, we get the aggregate supply curve of loanable funds *S′S′* which is normally very elastic. The horizontal distances between this curve and the *SS* curve measure the amounts of bank money which the banks are prepared to create or to cancel in the course of the day concerned at the various short-term interest rates. The short rate will therefore be determined by the intersection of the *S′S′* and the *DD′* curves. If the *SS* curve also run through the point of intersection of these two curves, the banks are merely renewing loans and maintaining the existing amount of money, and the stream of money expenditure will remain stable.

Now suppose owing to some technical inventions, the inducement for long-term investment has improved. The demand to borrow loanable funds to finance investment expenditures will therefore rise. If the entrepreneurs are seeking to finance their additional investment by short-term funds (with the hope of repaying them by floating long-term bonds or shares later on), this will directly be reflected in a shift of *DD′* to the right. But we shall assume that the entrepreneurs will go to the long-term capital market for the finance funds of their long-term investment projects. The demand for loanable funds or the supply of new securities on the capital market will then be increased. If the speculators in the capital market succeed in preventing the long-term interest rate from rising by taking up the additional supply of securities, the excess of the demand for investment finance fund over the inflow of gross savings on the capital market will have to be provided by the speculators, who, as we have seen above, will raise it chiefly through their brokers from the short-term money market. Thus the additional demand for investment finance funds is transferred by the speculators from the capital market to the money market and the *DD′* curve will be shifted to the right just as if the entrepreneurs had attempted to raise the " finance " from the money market in the first instance.[2] If the short rate is allowed to rise to the point where the new demand curve $D_1D_1'$ intersects the *SS* curve, the stream of money expenditures will not rise in spite of some increase in investment and speculation on the capital market. However, as the supply of bank credit is elastic, the short rate will only rise to the point where $D_1D_1'$ cuts the *S′S′* curve. Thus short-term lending will

[1] According to Mr. Kaldor, the elasticity of the supply of money in a modern banking system is ensured partly by the open market operations of the central bank, partly by the commercial banks not maintaining a strict reserve ratio in the face of fluctuations in the demand for loans, and partly it is a consequence of the fact that under present banking practices a switch-over from current deposits to savings deposits automatically reduces the amount of deposit money in existence, and vice versa. (Kaldor, *loc. cit.*, p. 14, footnote 1.)

[2] It is possible that not all the extra funds borrowed on the long term capital market will be needed to finance expenditures during the day. Consequently part of the funds will flow back to the money market and thus shift the *SS* curve to the right. However, the relending of the extra funds borrowed on the capital market will always fall short of the amount borrowed by the fund needed to finance investment expenditures to-day. Therefore *SS* will always be shifted less to the right than *DD*. For the sake of simplicity, we assume that *SS* is not shifted, but *DD* is displaced to the right by the amount which the entrepreneurs retain to finance their investment expenditures during to-day.

The transference of the demand for liquid funds to the money market, i.e., the speculators' demand for short loans may be to some extent a function of the short rate that will be established on the money market. Its elasticity depends on the extent to which speculators' expectation of long rate will be affected by the rise of the current short rate and whether the rising cost of short loans may or may not deter the speculators from holding additional stocks of securities.

exceed the flow of savings into the money market and the excess $BM_1$ will be provided by the creation of new bank money, which will be transferred to the capital market by the speculators to finance the extra investment expenditures, and consequently the stream of money expenditures will increase.

Similarly when there is an exhaustion of investment opportunities and the speculators attempt to peg the long rate by selling old securities, the proceeds will be immediately transferred to the money market. This can be represented by a shift of the $SS$ and the $S'S'$ curve to the right (or alternatively a shift of the $DD$ curve to the left), and the inflexibility of the short rate or the elasticity of bank credit will ensure that part of the liquid funds absorbed by the speculators and transferred to the money market will be cancelled. Thus in normal circumstances the elasticity of bank credit appears to be the chief explanation of the instability of the money stream although the banks merely play a passive part. Hoarding of idle balance comes into force on an important scale only when the banks have allowed the rate of interest to fall to a very low level.

The essence of speculation in the stock exchange is therefore that it shifts the burden of adjusting investment to the supply of savings from the long-term capital market, where the rate of interest is comparatively more effective in regulating the flows of investment and savings, to the money market where the short-term rate of interest, even when it is not arbitrarily fixed by the banking system, is quite impotent in regulating the industrial demand for loanable funds and the supply of savings. It is impotent because the industrial demand for short-term loans, which are used chiefly to finance investment in working capital and stocks is very inelastic in relation to the short rate, as has been shown by the Oxford inquiries,[1] and the supply of savings is likely to be even less responsive to the short rate. Although it is always possible to stop expansion by letting the short rate rise to whatever extent required, when there is an exhaustion of investment opportunities and the bearish speculators transfer the vast excess of savings over investment at the capital market to the money market, the banks will not be able to lower the short rate infinitely to stimulate a corresponding increase in short-term investment; for when the short rate falls to a very low level, hoarding of idle cash will set in and make further reduction impossible. It remains true, however, for normal times, when banks are supposed to keep the short rate at a more or less conventional level and there is no serious change in the transaction demand for cash due to changes in business habits and confidence apart from changes in the volume of transaction, that speculation in long-term securities (or in any other commodities) leads to fluctuations in activities chiefly through indirectly inducing the banks to create additional bank credit or to cancel existing money.

Thus the modern discussion on liquidity preference and speculation seems to lead us back to the age-old doctrine of the early classical economists, e.g., Thornton, Ricardo and Joplin, that the explanation of income instability is the divergence between some sort of natural rate of interest and the rate charged by the banks and the consequent expansion or cancellation of bank credit. What we have discussed above appears to justify their preoccupation with the short rate as arbitrarily fixed by the banking system, which is shared even by Wicksell,[2] Professor Robertson and the modern Austrian School. For as we have seen above the inefficacy of the long-term capital market brings about fluctuations in activities only indirectly through its repercussion on the money market and only in so far as the latter is also ineffective.[3] It is when the money market has proved to be a broken reed, that our hopes for the automatic self-adjustment of our economic system have to be abandoned.

## II

In this section we shall make a simple attempt to examine the effect of speculation in the stock market on the volume of bank credit and the level of trade activity in the light of American statistics. If our theoretical analysis is more or less in conformity with the realistic situation, we shall expect to find some correlation between the net speculative buying or selling of securities and the expansion or contraction of bank credits and the ups and downs of business activity.[1] Of course very close correlation cannot be expected, for speculation on the stock exchange is not the only factor that affects the level of activities and the amount of circulating money. Other factors, e.g., the balance of trade, the granting of credits by banks directly to industry to finance investments, government deficit spending, etc., may reinforce or offset the effect of speculation on the capital market. In our present investigation, however, we shall make no attempt to eliminate the effects of these other factors. If in spite of the interference of other factors some correlation can be detected between the variations of speculative stock-holdings and the level of activities and the amount of bank credit outstanding, the result will be at least of suggestive value.

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[1] See P. W. S. Andrews, "An Inquiry into The Effects of The Rate of Interest," and R. S. Sayers, "Businessmen and The Terms of Borrowings", *Oxford Economic Papers*, III.

[2] Although Wicksell mentioned explicitly at some places that it was the long rate that should be equated to the natural rate (see *Lectures on Political Economy*, Vol. II, p. 191), yet he accounted for the inflexibility of the money rate of interest by the arbitrariness of the rate charged by the banks and the elasticity of bank credit.

[3] There seems therefore to be little ground for the apprehension, as expressed by Dr. Lachmann, that inelastic expectations with regard to the long-term interest rate would vitiate the Wicksellian doctrine that booms and slumps are engineered by banks fixing the money rate of interest below or above its natural level. (See L. M. Lachmann, "The Rôle of Expectations in Economics as a Social Science," ECONOMICA, Feb., 1943, p. 23.)

It has been pointed out by Dr. Eiteman that the net amounts of speculative buying or selling of securities, which reflect the excesses (positive or negative) of the outflow of liquid funds into the industry from the stock market over the inflow of current disposable income thereinto, can be gauged to some extent by the variations of the volume of brokers' loans.[2] For the variation of brokers' borrowings depends on the difference between the payments in by customers who have bought shares (plus dividends received on securities held for the customers) and the withdrawals by customers who have sold shares (or are collecting dividends). Provided that the speculators always speculate on " margin ", instead of through their brokers, and do not go directly to the money market or banks for funds to finance their purchases, or in the case of selling, withdraw the proceeds from the brokers in order to transfer them to the money market on their own account, instead of leaving the proceeds with the brokers in repayment of the margin debts, the variations of brokers' loans would give us an indication of the variations of the speculative stocks on the stock market. For the selling of bearish speculators, who use the proceeds to repay their margin debts to the brokers, will enable the brokers to discharge their own debts to the money market and the banks to the same extent, and the buying of the bullish speculators who buy on new margin debts, will oblige the brokers to borrow new loans to pay for their customers' purchases. The increase or decrease of the total brokers' loans will therefore indicate whether the speculators as a whole are adding to or diminishing their holdings.[3] The proviso which has to be made above, viz., that the speculators always raise their liquid funds from the money market and banks or return the proceeds of their sales to the money market and banks through the agency of the brokers, is perhaps not altogether a very unrealistic one, in view of the prevailing practice of the speculators (the professional ones at least) to speculate on margin.[4] Therefore the variations in brokers' loans may perhaps provide us a rough indication of the inflationary or deflationary effects of speculation on the stock market.

If, however, the speculators do sometimes borrow funds directly from the money market or mobilise their own savings deposits when they buy securities, the increase in brokers' loans may no doubt underestimate the inflationary effect of their speculative buying. On the other hand, if the speculators on selling their securities withdraw the proceeds from the brokers and use them to pay off their own short-term debts, or to grant loans to the money market or to buy short-term bills, the deflationary effect of their selling will not be reflected in a decline in brokers' loans. Furthermore, if when the opinions of the speculators are divided, the bullish speculators raise their funds directly from the money market (e.g., by borrowing new short-term loans, or selling the bills in their possession, or mobilising their savings deposits) to buy from the bears who use the proceeds, however, to pay off their margin debts thus enabling their brokers to repay their own outstanding debts, brokers' loans will decrease, although there is no net speculative selling of securities. In other words, brokers' loans may decline without there being any absorption of current savings by the speculators. On the other hand, if the bullish speculators buy on margin from the bears, who withdraw the proceeds, however, from the brokers in order to invest on short-term assets on their own account (i.e., to repay their short-term debts, or to lend out on call loans, or to put the money on bills or savings deposits), brokers' loans will rise although no net speculative buying of securities has occurred. In this way, brokers' loans may rise without there being any excess of outflow of funds into industry from the stock market over the inflow of current income into the stock market.

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[1] The term speculative selling of long-term securities, as it is employed in our theoretical discussion, means the selling of securities not for the purpose of financing investment or consumption expenditures, but for the purpose of transforming the capital into more liquid types of assets. And the term speculative, buying means in our sense the purchase of securities not with the current disposable income (gross) but with the accumulated liquid capital or borrowed capital.

[2] Wilford J. Eiteman, "The Economics of Brokers' Loans," *American Economic Review*, 1932, also " The Economic Significance of Brokers' Loans ", *Journal of Political Economy*, 1932.

[3] Lord Keynes, therefore, appears to have been misled by false scent when he maintained that " an increase (in the volume of brokers' loans) meant that the rise of security-prices had gone still further beyond the point at which it would be just enough to offset the bullishness of average sentiment, in the sense that it had led to an increase in the ' bear ' position ". (*Treatise*, Vol. I, p. 251.) On the contrary, as pointed out by Dr. Eiteman and as we shall see from our statistics, the increase in brokers' loans during the prosperous years of the twenties appeared to imply that the rise of security-prices was not sufficient to check the average bullish sentiment, so that the speculators as a whole were adding to their holdings and had to borrow on the short-term loan market through their brokers to pay for the purchases. It was in fact the first of the four types of speculative markets enumerated by Lord Keynes. (*Ibid.*, pp. 252–3.)

[4] The speculators may no doubt use their current incomes (e.g., dividends on their holdings of securities) to buy securities or to put up their margins. However, in so far as they purchase securities with current incomes, their purchases are not speculative buying in our present definition of the term. Speculators' current incomes paid in to reduce their margin debts may be considered as current savings flowing into the stock market. They tend to diminish brokers' loans outstanding, unless offset by other speculative buying of securities on margin.

Considerations similar to those given above led Professor Machlup to the rather extreme conclusion that the variations or brokers' loans can tell us absolutely nothing.[1] Whether or not these possibilities are in fact important enough to vitiate the variations of brokers' loans even as a rough indication of the excesses (positive or negative) of investment borrowings over the inflow of current savings on the capital market, i.e., whether the assumption that speculators generally buy and sell on " margin " and do not go indirectly to the money market or banks for funds or for investing the sale proceeds is realistic enough as a first approximation, cannot be judged *a priori*. In our present study we shall tentatively employ the monthly figures of brokers' loans as an index of the variations of speculative stocks on the capital market. If the variations of brokers' loans are found to correlate to some extent with the fluctuations of activities and the volume of bank credit, there will be then a *prima facie* case for Dr. Eiteman's interpretation of the significance of brokers' loans.

In the following chart we shall compare the monthly variations of brokers' loans by New York City banks (" for account of banks " and " total ") with the variations of net demand deposits of the reporting member banks in the leading cities and the index of industrial production. The limitations of our statistics are at once obvious.

(1) Brokers' loans by New York City banks may not be representative of brokers' loans for the country as a whole. Loans to brokers outside New York City have to be neglected for lack of monthly figures. Furthermore brokers' borrowings from other sources than banks and the money loaned out by brokers on the money market other than to other brokers or their clients have also to be neglected for lack of adequate statistics. Fortunately these are very small as compared with brokers' loans from banks.

(2) The demand deposits of the weekly reporting member banks in leading cities may not be representative of the total amount of the demand deposits and currency of the whole country.

(3) As is clear from our theoretical discussion, the excess of investment borrowing over the supply of current savings at the capital market affects in the first instance the aggregate private money outlay. The relation between the money expenditure and the physical output, however, may be disturbed by changes in prices and costs.

Nevertheless, our statistics seem to reveal some interesting correlations between the fluctuations of brokers' loans and the fluctuations of demand deposits and output, which cannot be dismissed as mere coincidence, and which appear to render some support to our argument.

There is a quite noticeable degree of synchronisation between the variations of brokers' loans and the variations of demand deposits from the beginning of our chart up to the first half of 1924. The sharp rise of demand deposits during the latter half of that year, which was probably due to the open market operation by banks and to the import of gold, and the subsequent fall in the opening months of 1925 are, however, quite out of proportion with the changes in brokers' loans. During 1925 and 1926 the relation between these two series is not very definite. From the end of 1926, however, the fluctuations of brokers' loans and demand deposits synchronised to a remarkable extent. Although since 1928 the brokers' loans total began to rise to an unprecedented height leaving brokers' loans "for account of banks" and the demand deposits behind, the two latter still maintained a remarkably close correlation up to September, 1929.

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[1] See Machlup, *op. cit.*, Chap. VII, especially pp. 123–128.

The lack of correlation between brokers' loans and demand deposits since the October of 1929 was probably due to the special conditions of the crisis and the special monetary measures adopted by the authority, which entirely overwhelmed the relation between brokers' loans and demand deposits as we should expect from our previous argument. For instance, the sharp rise of demand deposits in October and November was no doubt brought about by the distress borrowing at the collapse of the stock market boom and the desperate effort of the member banks to come to the aid of the stock market. And the fact that demand deposits remained at a high level until September, 1931, after the drastic decline of production and the wholesale liquidation of brokers' loans was probably because the risk of default of short-term assets had increased so much that people fled to cash and demand deposits until even deposits came to be distrusted, as again before the banking crisis in the beginning of 1933.

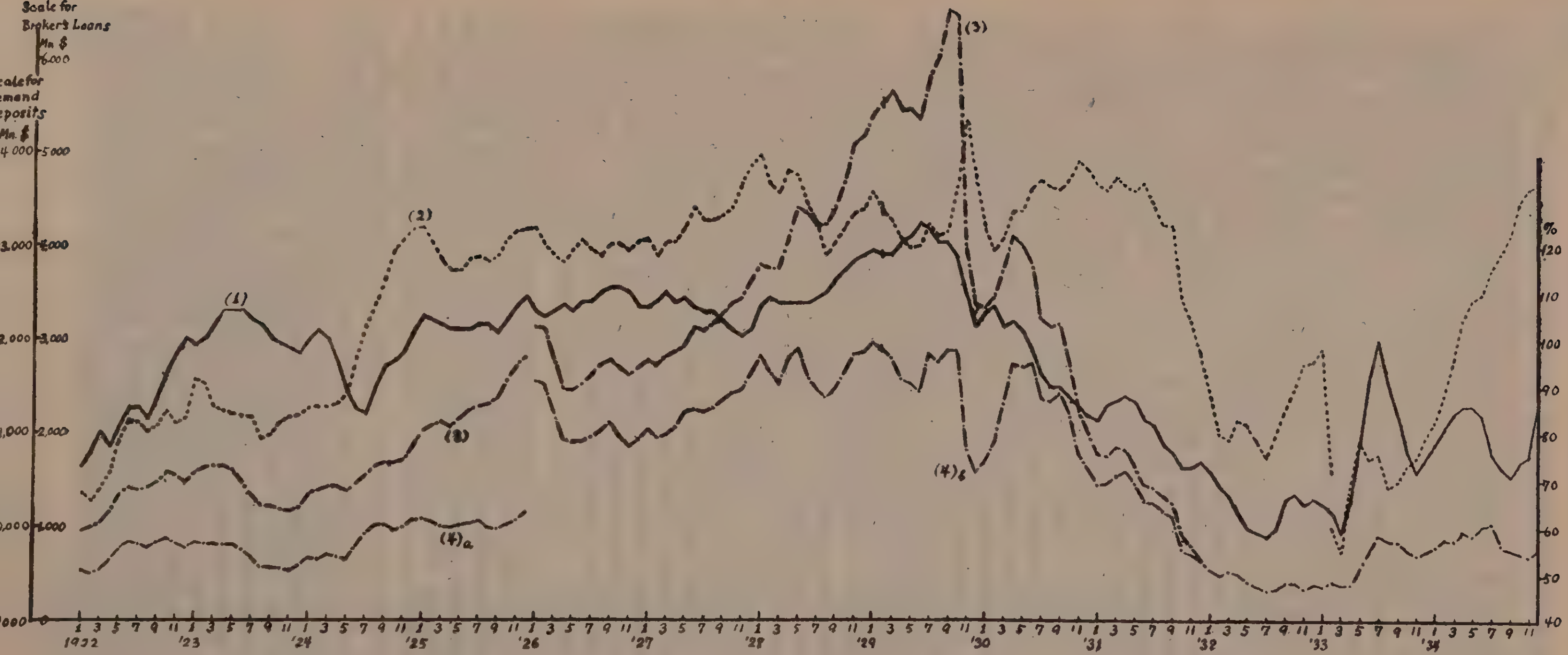
A positive correlation between the index of industrial production and brokers' loans is also observable for some periods in our chart. During 1922 and 1923, the fluctuations of the index of production seemed to correspond with the variations in brokers' loans. But the sharp decline of production in the middle of 1924 was not sufficiently accounted for by the slight dip in brokers' loans. One explanation for this is that the recession was chiefly brought about by disinvestment in inventories which were generally financed not by stock market credit but mostly by short-term credits. Moreover the index of physical output might have exaggerated the decline in money expenditure, for the national income in terms of money was actually higher in 1924 than in 1923. The rising trend of brokers' loans undoubtedly contributed to the recovery of production in the second half of 1924 and in 1925. In 1926 the relation was not very clear and the recession of 1927 was not reflected in brokers' loans, which were rising during that year. Since October, 1927, however, the index of production seemed to follow the lead of brokers' loans total and resumed its rise, reaching a peak in June, 1929, three months behind the peak of brokers' loans in March, 1929.

The rise of the index of production together with brokers' loans total, relatively to demand deposits, indicates that part of the speculative buying of securities, which conduced to the rise of activity, was financed not by additional bank credit but by a speeding up of the velocity of circulation of demand deposits. This was probably because after the long period of prosperity of the twenties, business men became more confident in outlook, so that money substitutes, e.g., book credits, commercial bills, etc., were used and circulated to a greater extent and consequently less money was required for a given volume of transaction. It might also be partly due to the fact that the very high short-term rate of interest prevailing then might have induced business men to keep less cash in our strict sense for precautionary purposes, thus making more cash available to the stock market. The explanation that this might have been the result of the dishoarding of speculative balances by the speculators is, however, out of question, for, as we have seen, speculators do not as a rule keep idle balances in demand deposits.

The resumption of the spectacular rise of brokers' loans total from July to September, however, seemed to have hardly any effect on production, suggesting that the speculative spirit of the bulls had so completely run wild that the bears sold their holdings but, in order to take advantage of the very high rate on call loans, relent the proceeds to brokers who used them in turn to finance the purchases of their bullish clients. In this way brokers' loans soared up possibly without there having been any real afflux of funds out of the stock market. And consequently industrial production was hardly affected.

The sharp decline of production set in only in November when brokers' loans were drastically liquidated, and the short-lived recovery of production early in 1930 seemed to coincide with the revival in brokers' loans. After that the downward trend of output seemed to correspond with the similar trend of brokers' loans.

In conclusion, we may say that our chart, crude as it is, seems to reveal, for certain periods at least, some interesting correlations between the variations of brokers' loans (which we have taken as a rough indication of the discrepancies between investment demand for funds and the current supply of savings at the stock market), the demand deposits and the index of production. It appears to afford some factual support to our contention that in normal circumstances, speculation in securities on the capital market brings about fluctuations in income and activity mainly by shifting from the capital market to the money market the excess demand or supply of liquid funds which the banking system meets by expanding credit or absorbs through credit cancellation as the case may be.



THE MONTHLY VARIATIONS OF BROKERS' LOANS, DEMAND DEPOSITS AND INDEX OF INDUSTRIAL PRODUCTION

(1) Index of industrial production, 1923—1925 average = 100, adjusted for seasonal variations. Source: *Federal Reserve Bulletin.*

(2) Demand deposits. 1922—1930, net demand deposits of weekly reporting member banks in leading cities, monthly averages of weekly figures. Source: *Federal Reserve Bulletin.* 1931—1934, figures for weeks ending Wednesday nearest end of month. Source: *Royal Economic Society Memorandum.*

(3) 1922–25, Brokers' Loans—Street Loans (on Call and on Time) by reporting member banks in N.Y. City, total. 1926—1934, Loans to Brokers and Dealers (on Call and on Time) secured by stocks and bonds, made by reporting member banks in N.Y. City, total. (monthly averages of weekly figures). Source: *Federal Reserve Bulletin.*

(4) (*a*) 1922–25, Brokers' loans by N.Y. City member banks for own account; (*b*) 1926—1934, Brokers' loans by N.Y. City member banks for account of banks.

# The Plans for an International Clearing System

By E. Victor Morgan

The British "Proposals for an International Clearing Union" and the American "Proposal for a United and Associated Nations Stabilisation Fund" have now been before the public for some time. They have been debated by both houses of Parliament, and widely discussed in the press. It should, therefore, now be possible to take a general view of them, to discern what they have in common and what are the differences between them, and to assess the contribution which each, or a combination of the two, might make in the post-war world.

Both plans clearly have their origin in what the writer of the British proposals calls "the climate of opinion" which now exists on such matters. This climate has changed greatly in recent years, and one of the chief reasons for the change has been the realisation that, in a world subject to economic fluctuations and with moderately free international trade, stability in the foreign exchanges can be had only at the cost of instability in interest rates, prices and employment.

Before the last war, this dilemma appeared unimportant. Economic activity was generally expanding, and unemployment was moderate or low; this country was both the principal creditor and the unchallenged monetary leader of the world; we had only to slacken our rate of lending for gold to pour in from the rest of the world in discharge of previous commitments; yet this process did not press too hard on the debtor countries, because a quite small influx of gold to London soon exerted, through the policy of the Bank, a marked expansionist influence on British economy. Yet even so, there were many frictions to interfere with the smooth working of the gold standard, and a few cases in which crisis and depression, originating in one country, spread with great rapidity through the whole industrial world.

After 1918 things were much more difficult. The war and the peace left large outstanding payments between nations; wide divergences in prices and wages had not been balanced by changes in exchange rates; New York rivalled London both as creditor and monetary leader, and the two centres by no means always worked in harmony; political troubles added to economic ones produced flights of "hot money" from one centre to another; the rate of capital investment, in some countries at least, was declining, and slumps became worse than ever before, while even in booms unemployment was severe. Countries which were losing gold were forced to add deflation to their other troubles, while in the countries which received it, gold often failed to produce the corresponding inflationary tendencies. Herein was a marked difference from the pre-war period, and one which meant that the whole burden of adjustment had to be borne by the debtor country. Thus the dilemma was clearly revealed, and the gold standard, as practised in the post-war world, stood accused of forcing its servants into policies causing internal instability, and of spreading the virus of depression.

Once the nature of the dilemma was apparent, various methods of escape were tried. Some countries obtained temporary relief by short-term borrowing; some cut adrift from gold, sacrificing stability in foreign exchange in the hope of finding stability in their domestic economy, but exchange depreciation proved a double-edged weapon; some tried, with a good deal of success, to protect themselves, at least against short-run fluctuations, by means of exchange equalisation accounts; almost all were driven to impose an increasing number of direct restrictions on trade; finally, some tried to withdraw into a sheltered backwater, away from the main stream of international affairs, by means of rigid exchange control and bilateral clearing arrangements. Thus the world was deliberately depriving itself of the benefits which accrue from international division of labour while, in all but a few countries, depression recurred and unemployment continued.

This is the experience which formed the present " climate of opinion ", and from the repetition of which the present plans seek to deliver the world.

Being formed by such a common experience, it is natural that the two schemes should display many similarities. Both aim at stabilising exchange rates and avoiding bilateral clearing agreements; both set out to achieve this with the minimum of internal instability for their members; both hope by this means to prepare the way for greater freedom of international trade. Both the proposed organisations would deal only with central banks or government agencies, and not directly with the market, and there is substantial similarity between the provisions for their government. When we get down to details there are further similarities, but they are interwoven with a number of differences, differences of which it is important, though sometimes difficult, to grasp the precise significance.

Very early in the proposals we come across one difference which appears, at first sight, fundamental. One of the objects of the American plan is stated to be, " To *shorten the periods*, and lessen the degree of disequilibrium in the international balance of payments of member countries ". By contrast, the British author stresses the importance of the *increased* time allowed for adjustment. It is a very general truth that the more quickly a change has to be made the more disturbance it is likely to cause. Hence if this American statement were to be taken at its face value, it would be highly disturbing. Fortunately, as we suggest later, we need not attach too much importance to it.

Another difference is in the nature of the international monetary unit. " Bancor " is a real currency, which has general acceptability among members, and which passes from hand to hand, or rather from book to book, in settlement of debts. " Unitas ", on the other hand, is simply a unit of account, actual transactions taking place in the currencies of member states. Yet " bancor ", though in this sense a real currency, is nevertheless entirely fictitious. The system works wholly on overdrafts, which the clearing union can give without holding any reserves, because they are inconvertible, and which members can use in settlement of debts because every other member is pledged to accept them. Under the American plan, on the other hand, the stabilisation fund is equipped at the beginning with a " capital " in gold, securities of member states and member currencies.

Under the British scheme, a country with an active balance of payments accumulates credits in " bancor ", while one with a passive balance overdraws its account. Under the American plan, a country with a passive balance buys the foreign currency required to settle its debts in exchange for its own currency (in excess, of course, of its initial contribution), while the country with an active balance receives back, indirectly, its own currency originally contributed to the fund. Thus the fund will hold more currency of a debtor country, and less of a creditor, than was originally paid in. These differences have an interesting effect on the plan of operation of the respective organisations.

Both proposals involve the use of a quota, but in determining quotas two further differences arise. The British formula depends solely on the volume of international trade of member states. The American formula would take account, not of the absolute volume of trade, but only of its fluctuations, and would also give weight to the national income and the holdings of gold and foreign exchange of members. Obviously the United States itself would get a considerably larger relative quota under this formula and, in view of the violent fluctuations from which that country has suffered, this might well prove useful in practice.

Perhaps more important is the difference in the prospective size of the quotas. The American plan contemplates a total fund of " at least five billion dollars ", or about £1,250 million at present rates of exchange. The British suggestion is 75 per cent. of the average of imports and exports for the three pre-war years, with provisions for subsequent adjustment. By this formula, the United Kingdom alone would have a quota of over £950 million, and the total quota for the world would approximately be seven times the American figure.

These differences in the structure of the two organisations are bound to lead to differences in operation, though the fundamental purpose is, of course, the same—to supply any country which is temporarily in debt to the rest of the world with the means of payment, without causing any immediate disturbance to its internal economy.

Let us consider first the case of a creditor nation. As we have seen, the American stabilisation fund pays out the currency of such a country. The fund's stocks of currency are limited, and it is, therefore, necessary to provide against their exhaustion; hence the provision for the rationing by the fund of scarce currencies. We may assume that central banks would not resort to the fund until normal sources of exchange had been drawn on to the full. Hence rationing would automatically cause some restriction of the exports of a creditor country. After it had reached the rationing stage, a country could only continue to have an active balance of payments in so far as its exporters would sell on credit, or foreign importers could buy gold from their central banks or " black market " exchange at above the legal rate. However, before rationing comes into operation, the directors of the fund are bound to report on the circumstances, and the member to give this report sympathetic consideration, and this would presumably often lead to changes in policy which would make rationing unnecessary.

The British clearing union has no need to contemplate rationing at all for, short of the combined quotas of all the rest of the world, there is no limit to the amount of " bancor " which can be transferred to any one account. But if a creditor country went on amassing balances without any change of policy, we should have a situation like that arising, under the gold standard, when a receiving country " sterilises " its gold imports. The whole burden of adjustment would be thrown on the debtor, which is one of the very things the plan seeks to avoid. Hence the provision that, when a member's credit balance, on the average of two years, has exceeded half its quota, that member must consult with the board on ways of reducing the balance. This is closely parallel to the American directors' report but, owing to the greater size of the quotas and the longer time involved, the British scheme is the more flexible.

When it comes to dealing with debtor countries, the size of the quotas alone would necessitate a difference in procedure. Under the American plan, a member has the *right* to purchase foreign exchange up to the full amount of its quota, subject only to the directors' power to impose special conditions, " when a country is exhausting its quota more rapidly than is warranted ". When the fund's holdings of any currency exceed the member's quota, a special reserve must be provided, and the fund may not hold more than 200 per cent. of a quota in any currency without the approval of a four-fifths majority of the board.

Now a country will normally pay up 12½ per cent. of its quota in its own currency; hence, when it is in debt to the extent of 87½ per cent. of its quota, the fund's total holding of its currency will be 100 per cent, and a special reserve must be deposited. But the country has a right to make purchases of foreign exchange up to another 12½ per cent of its quota (bringing its total purchases to 100 per cent.) and a conditional right of purchase up to a further 75 per cent. (bringing the fund's total holding of its currency up to 200 per cent.) Thus the normal upper limit to any country's purchases of exchange is 187½ per cent. of its quota.

The British quotas being so much bigger, it is natural that the right of members to use them to the full should be more restricted. Thus a member may not increase its debit balance by more than ¼ of its quota in a single year without the approval of the board. When a debit balance reaches half a country's quota, the board may require the deposit of collateral and a measure of exchange depreciation, the control of capital exports, or the surrender of separately held gold or foreign exchange. The board may also recommend internal measures and, when the debit reaches three-quarters of the quota, it may require them. In this case, failure to comply within two years leads to the offender being declared in default. Again the British plan, with its larger quotas and more gradual application of pressure, appears the more elastic.

Both plans envisage the control of capital exports, and the setting up of an international organisation to supply long-term capital to countries which need it. But they differ on the question of exchange control. The British writer regards this as desirable, though not essential, but the American looks forward to the removal of existing controls. The difficulty of controlling capital exports without some form of exchange control has often been referred to, and in practice it seems likely that the two will be found inseparable.

In other details of working—expenses, a small management charge adjusted so as to discourage large balances, either positive or negative, the collection of statistics, dealing with defaulters, etc.—there is substantial agreement.

It remains to mention the American proposals for the liquidation of abnormal war balances, which have no counterpart in the British plan. The basic provisions of this section are that abnormal war balances shall be notified by members on joining the scheme; that the fund shall, from time to time, announce the amounts of these balances which it is prepared to purchase; that purchases shall normally be made by the fund in the currency of the creditor country; and that *both the creditor and the debtor* shall undertake to repurchase 40 per cent. of the sum taken by the fund, in gold or such currencies as the directors may require, at the rate of 2 per cent. a year for twenty years, beginning three years after the initial purchase. Holdings of currency thus acquired are, of course, outside the normal quota provisions. The creditor thus has an easy means of turning foreign balances into domestic currency, the debtor, a method of meeting its obligations without derangement to its own monetary system. The debtor becomes in debt to the fund instead of the original creditor, while the creditor receives back its own currency from the fund. The fund's holdings of creditor currencies are thus diminished, and if such a country has an adverse balance of payments, it can buy that much more foreign exchange without coming up against the quota restrictions. If, however, the creditor has a favourable balance of payments, the time when it will come up against the rationing restrictions is only hastened, and in that case the scheme might not prove an unmixed blessing for the creditor.

When it comes to the ultimate settlement of the debt, 20 per cent. must be held for 23 years by the fund, with exceptions in special circumstances, after which it can be disposed of as desired. The 40 per cent. repurchased by the debtor is similar to the repayment of any other international debt, except that it is deferred. The novelty is in the 40 per cent. repurchase by the creditor, who will have to pay, not in its own currency, but in gold or foreign currencies, and also to pay a small management charge in gold. The balance repurchased will, of course, remain an outstanding claim of the creditor on the debtor, to be settled ultimately either in gold or in commodity exports. The chief advantages of this rather complicated arrangement are that it would give creditor countries an incentive to develop their trade with their debtors, and that it would tend to check an undue accumulation of gold by creditor countries.

Having thus anatomised the two schemes, and compared in detail their similarities and differences, we may conclude with a few more general remarks. Perhaps the most obvious point which has emerged from our discussion so far is that the similarities between the two plans are much more fundamental than their differences. This does not mean that the differences are unimportant, but it does mean that it should prove quite possible to reconcile them.

It has often been said that the British plan is more elastic than the American, and also that it is more expansionist. With the first, we have shown our agreement already, but the second requires some comment. The British plan may be more expansionist because it requires no initial deposit from members. The deposits of gold and local currencies required by the American plan will, it may be argued, be withdrawn from domestic circulation, and so exert a deflationary influence. But whether this is so or not will depend entirely on the monetary policy of members. They may see fit to alter their laws governing gold reserves and the creation of local currency in such a way as to offset this tendency, or they may not. On the whole it seems desirable that they should, and it would certainly be desirable that agreement should be reached on this point between members before the scheme started.

Further, it might be argued that the American plan would be more deflationary in that a debtor country must withdraw some of its own currency from circulation or reserves in order to purchase foreign exchange from the stabilisation fund, whereas, under the British plan, transfers of " bancor " are entirely independent of domestic currency, adjustments of which are left to the discretion of the monetary authority concerned. Again the effect of this difference would depend very much on the currency laws and banking policy of individual states.

The British system of a super-national currency has some advantages over the American one of dealings in the national currencies of members; it makes transfers independent of local monetary conditions, and it obviates the need for rationing. Further, the larger initial quotas, and the correspondingly more gradual application of pressure are big advantages of the British proposals. On other matters, however, substantial concessions could well be made to the American point of view. The war balances provisions could be accepted, as could some modification in the formula for the initial determination of quotas, while Lord Keynes has pointed out that there might be advantages in the " backing " of overdrafts by the securities of member states. On some such lines as these, a good compromise might be reached.

It remains to ask what place such a scheme would have in the post-war world, and how it would help in the solution of the problems which we discussed at the beginning of this article. The authors of both plans obviously envisage the removal of a number of difficulties which beset us in the inter-war years. The lease-lend agreements, and the present proposals for dealing with war balances are designed to simplify this problem; the control of capital exports should check the ebb and flow of " hot money "; the provision of an international organisation for the supplying of long-term capital, should reduce those fluctuations in its supply, which were a disturbing feature of the past. But all these are quite distinct from the main purpose of the present plans. There still remains the fundamental dilemma, the choice between stability in exchanges and stability in interest rates, prices and employment.

As we have seen, the present proposals come down emphatically on the side of stability in exchanges; in both it is obviously intended that rates once agreed upon shall be changed only in very exceptional circumstances. What the plans do provide, by contrast with the simple gold standard, is time for adjustment. Under the gold standard, an adverse balance of payments very quickly produces deflationary conditions at home, and one result of this is that economic depression spreads very rapidly from one country to another. But an adverse balance of payments under the British plan produces no immediate or automatic deflationary effect, and under the American, very little. This effect can, if desired, be postponed for several years. Thus if an adverse balance is due to temporary causes, it may be possible to avoid internal pressure altogether; if the causes are more permanent, the pressure can be applied gradually. In this the present plans are a logical development of the function which has been performed in the past by central banks (permitting some variation in their cash ratios) and later by exchange equalisation accounts.

This time factor may be very valuable in checking the spread of slumps. However, if any country has an adverse balance because its wages and prices are permanently out of line with others, then it comes up against the old dilemma again. The acceptance of either of these schemes would practically rule out exchange depreciation as a solution. Hence prices must fall where they are " too high " or rise where they are " too low ". The aim of the plans is that both movements should take place simultaneously, the adjustment being shared between debtor and creditor, as it would be under an ideal gold standard.

The prospects of success depend, of course, very much on the actions of member states, on their willingness to pursue whatever policy, either inflationary or deflationary, which will make things easier, rather than more difficult, for others. Of equal importance is the policy of individual states with regard to the trade cycle. The plans, as we have seen, provide a certain insulation against the spread of depression, and thus, if a country can check a depression in its early stages, it need never be passed on. If, however, it is allowed to run its whole course, it will almost certainly prove too strong for the barriers provided. Thus misguided or selfish action on the part of one important member might have serious repercussions for all the others, and even jeopardise the success of the whole system. Success will not be attained merely by setting up international machinery, though that is a necessary condition, but only by continued goodwill, and constant vigilance in all the many matters connected with its working.

# Professor Hayek's Theory of Interest

By F. A. Lutz

In the following discussion attention will be concentrated on that part of Professor Hayek's *Pure Theory of Capital*[1] which contains his theory of interest in " real " terms. That is to say, almost one half of the book (from chapter XVII onwards, with the exception of chapter XIX) will be neglected. This second half of Hayek's study, containing the theory of capital accumulation and of the influence of money on interest rates, is in my opinion excellent. But since it is the first part of the book which raised the most questions in my mind, I shall select that part for comment. The discussion of the theory of capital has become so involved that students of the problem now have considerable difficulty in understanding each other's language. It would therefore not surprise me if my interpretation of Hayek's theory were not always accepted by Hayek himself as the correct one. I can only plead that I went to considerable trouble in my attempt to understand his theory of interest. Before entering into a discussion of the latter, we must take up three of the concepts which Hayek uses in his theory.

(1) The Capital Concept. Hayek identifies capital with the aggregate of all non-permanent resources. This definition imposes upon him the necessity of drawing a line between what is permanent and what is not. Where this line falls depends, according to Hayek, on " the opinion of the economic subject as to whether particular resources at his command will last throughout the period in which he is interested (be it his lifetime or a longer period) or whether they will be exhausted or used up earlier than this " (p. 51 f.). This makes the capital concept dependent on the subjective attitude of the individual.

A closer examination of the concept shows that Hayek includes under " capital " the " produced means of production " (with the exception of those which can be considered as permanent, such as certain improvements to the land), consumers' goods existing at the moment, and natural resources, in so far as they are " wasting assets ". Hayek, like his predecessors, contrasts this " capital " with the " current input " by which he usually understands the current " pure " input, consisting in the current services of the permanent resources. (Although Hayek definitely prefers the more abstract term, I do not think that we commit a grave mistake if we identify the latter with the old-fashioned categories of " land and labour ".) Now Hayek is, of course, well aware that the services of the non-permanent resources are also " invested ", and he promises to use the term " mixed " input where the latter are intended to be included. But that term is not in fact used, and the contrast between non-permanent resources and current input which occurs all through the book seems to imply that there is some fundamental difference between the services of non-permanent resources and those of permanent resources. Such a difference does not, however, play any role in Hayek's system. It would seem, therefore, much more consistent to include under " current services " all the services that are rendered during the current period no matter whether they come from non-permanent or from permanent resources, and to designate as " capital " the resources themselves which render those services. (In the case of non-durable goods the distinction between the service of the good and the good itself is logically still valid although, of course, the specific form of the resource is changed with the rendering of the service.) As it is not customary to capitalise the services of labour, it is advisable explicitly to exclude labour from the capital concept.

(2) The " Input " and " Output " Functions. When describing the process of investment in his earlier publications, Hayek made use of what he would now call " the inverted input function ". In Diagram 1 time is measured along the ordinate, with the present moment at $O$ and the time axis pointing into the past, and the physical

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[1] F. A. Hayek, *The Pure Theory of Capital* (Macmillan), 1941.

input is measured along the abscissa. In the case where the investment of input is made continuously but the output matures at a moment of time (the continuous input—point output case), the curve *TR* will epresent Hayek's inverted input function. The distance between any point on the time axis and the corresponding point on the input curve (e.g., *KM*) shows the amount of input invested up to that time, and the slope of the curve at any point indicates the rate of application of input. If we measure input in value terms, we can draw another curve $TR_1$ such that $KM_1$ shows the amount of input plus accumulated interest up to the date *K*. $OR_1$ is then the value of the total output, $RR_1$ is that part of the value of the output which is due to accumulated interest, and the slope of the curve represents the rate of investment of capital (the amount of capital at each point includes the accumulated interest). The area $OTR_1$ is the value of the capital stock at any moment of time provided we assume a continuous process of production which is constantly being repeated. It is, of course equally appropriate, if not more so, to choose *T* as representing the present moment. *OR* will then show the total amount of input which will have been invested up to the future moment *O* when the input matures.[1]

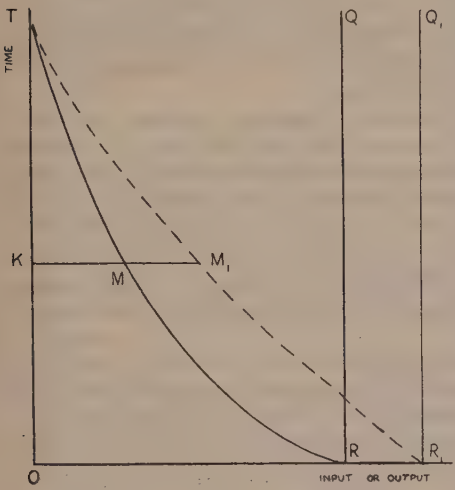

DIAGRAM 1.

These curves are easy enough to understand. But Hayek relegates them to the background in favour of the " input " and " output " curves (non-inverted). The mutual relationship between the two latter is quite difficult to grasp.

The input function is described as showing the range of periods for which we have to wait for the products of different units of a moment's input. We can use the same diagram as before. *OR* now represents to-day's input and the curve shows at what dates in the future the various units of to-day's input will ripen into finished products. The distance between any point on the curve and the time axis (e.g., *KM*) shows that part of to-day's input which will not yet have become output at the corresponding date in the future. The slope of the curve at any point thus shows the rate at which to-day's input will be ripening into finished products at the relevant future date. Whereas the " inverted " input curve shows all the inputs invested over the period *TO* which enter into the production of the *same day's* output, the curve under discussion here shows how the inputs of one day ripen into output at *different dates* in the future. The assumption which underlies the curve, viz., that we can find out when the different units of to-day's input ripen into consumers' goods, is rather heroic (as Hayek is aware). In all cases in which the creation of durable goods is involved (and there are practically no others) we cannot say when the various units of input of to-day will ripen into consumers' goods, because there is no way of ascertaining which particular input produces the service which the durable good yields at a particular date.

It is the output curve which creates most difficulties in the reader's mind. Using the same diagram as before, we now have to consider *OR* as representing the output which is the result of a moment's input. The present is again at *O*, and *OT* represents the passage of time from the present to the future. The curve is now supposed to show the range of different periods for which we have to wait for the different units of the *output* which are due to a moment's input. *KM* will be the part of the output (due to this moment's input) which has not yet matured at *K*. The slope of the curve with respect to *RQ* or *OT* shows the rate at which the output due to a moment's input matures over time.

It seems to me that the difficulty which the reader first experiences in understanding the output curve arises from the following fact. As *O* is the present moment and *OR* measures output, the reader instinctively thinks of *OR* as being to-day's output which would, of course, be the result not of a moment's (a day's) input, but of input spread over a period of time. It would have been simpler if Hayek had consistently defined $OR$ (as he occasionally does) as *to-day's input measured in terms of the output to which it will give rise.* It would then make sense to say that the point $O$ represents the present.

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[1] Cf. footnote in *The Pure Theory of Capital*, p. 119.

Only on the basis of this interpretation can we understand the graph in which Hayek draws the input and output curve in one and the same diagram (the above diagram can be used again). He here calls the outer curve the output curve and the inner curve the input curve. If $O$ is the present and $OR$ is to-day's input, then $OR_1$ makes sense only if we define it as to-day's *input* in terms of *the value of its final output.* $RR_1$ can then be considered as the accumulated interest.

As we remarked above, in the case of durable goods (e.g., houses) the input function cannot be drawn. Hayek thinks, however, that we can draw the output function in this case. Indeed, if we assume that the construction period of the house can be neglected, we can measure on the $x$-axis a moment's input expressed in terms of the output to which it will give rise and then draw a curve showing the successive dates when the units of output (the services of the house) materialise, i.e., we can draw the output curve. But if we do not neglect the construction period we cannot draw an output curve, since we do not know what part of the output is to be ascribed to the input of a particular date. In most practical cases we can therefore draw neither an input nor an output curve.

It is not clear why Hayek more or less dropped the old " inverted " input and output curves in favour of these new concepts. It seems that he wanted at all costs to avoid looking " backwards " from the present into the past. But the " inverted " input curve may equally well be made to point forwards, as Hayek himself is aware. Moreover, it is obvious that if this new arrangement is intended to meet the objection that was first raised by Burchardt against the Austrian theory, viz., that we should never be able to find the beginning of a production process, that we should be compelled to make an infinite regress into the past, it fails in its purpose. For if we always look forwards only, the same difficulty presents itself in the form of an infinite progress into the future, i.e., in many cases an input may never mature. This problem is neglected in Hayek's book. All through his treatment of the interest problem he assumes that all inputs ripen into consumers' goods.

The input and output functions play a part in the analysis of the time structure of production, but they are of minor importance in the theory of interest. The old-fashioned productivity curves are the tools with which Hayek works there. And the input and output functions serve only as convenient instruments for classifying the cases which have to be analysed.

(3) THE CHOICE OF THE INVESTMENT PERIODS. We shall next examine the choice of the investment periods of inputs in the two simplest cases which Hayek analyses, proceeding on Hayek's assumption that the economy is directed by the will of a dictator who aims at a maximum income stream which remains constant over time. We shall suppose for the time being that the interest rate is given, i.e., either that the dictator has made up his mind about the rate of return he wants to achieve on investments, or else that the aim of reaching a maximised and constant income stream imposes a certain rate of return on him.[1] Hayek himself, in his treatment of the various cases, assumes the rate of interest to be given.

(*a*) *The point input—point output case.*

This is the case where an input invested at a given date matures into output at some later date, not fixed by technical conditions, without in the meantime requiring any co-operation from current input. The closest approximation to this case in practice is represented by the examples of wine and trees. For the time being we shall assume that the dictator has to deal exclusively with such cases. Here the input function is known, i.e., it is known when the inputs will ripen into outputs.

Hayek assumes that the dictator knows the productivity functions of the inputs with respect to time. He also assumes, that in the case where the society produces more than one good, the dictator knows the (relative) values of the commodities that emerge after the various possible investment periods, i.e., he knows the value productivity curves of the commodities (in such cases as wine we cannot even conceive of physical productivity curves since wine of different ages cannot be compared in terms of physical units).

We will postpone the discussion of the process of reaching equilibrium and will assume for the moment that the system is already in stationary equilibrium. In order for the output stream to be maximised, the condition that must be fulfilled according to Hayek is that the rate of increase of the product divided by the product must be the same for all maturing products no matter whether they mature at the same date or at different dates. (This rate of increase is the instantaneous rate of increase described by Jevons' formula for the interest rate $\frac{F^1(t)}{F(t)}$.)

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[1] Whether, or in what sense, this would be true will be discussed later.

Hayek depicts these conditions in two diagrams which we may combine into one. The curve $HP_1$ is a discount curve based on an arbitrarily chosen discount rate. We have to imagine, as was indicated above, that the dictator knows the rate of return he wants to obtain on his inputs. The investment processes with the productivity curves $OP$ and $OP_1$ respectively, starting at the same date $O$ but terminating at different dates ($L$ and $L_1$) will result in different products ($LP$ and $L_1P_1$), but both have to touch the discount curve though at different points if the rate of return on a unit of input invested at $O$ or $O_1$ be the same in both processes. The unit of input is assumed to be of the same kind in both lines of production and will, of course, have the same present value at the date of investment. The processes $OP_1$ and $O_1P_1$ starting at different dates but terminating at the same date, yield the same product, and touch the same discount curve, in this case at the same point. Here the inputs are chosen in such quantities as to yield the same product at the date $L_1$. The investment process starting at $O_1$ and ending at $L_1$ will require more input than the process starting at $O$ and ending at $L_1$.[1]

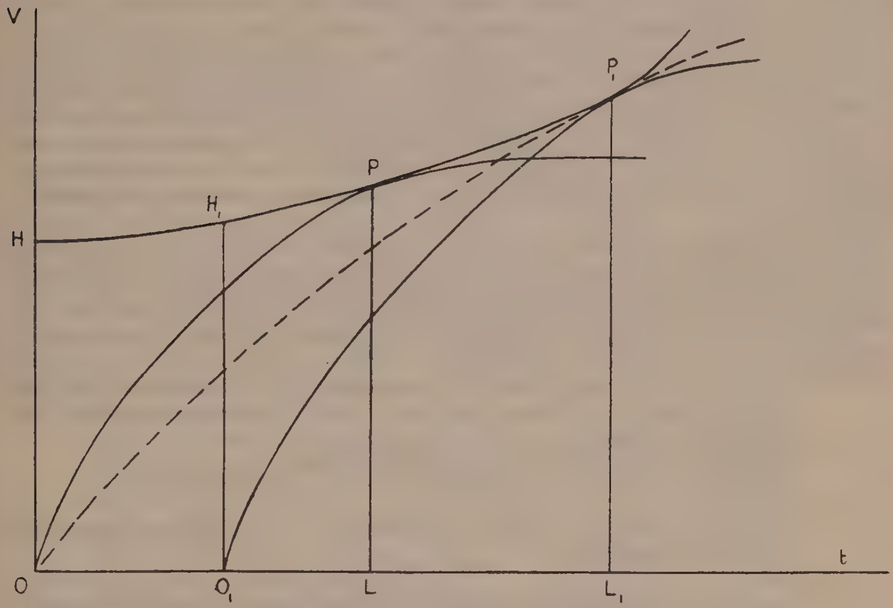

DIAGRAM 2

Hayek's treatment of this problem which concentrates on formulating the conditions determining the investment periods once the interest rate is known, calls for some additional comment.

(i) Even though the discount rate is supposed to be given, there are innumerable parallel discount curves reflecting this rate, some of which (namely those which are lower than the one drawn in the diagram) cut through the productivity curves. The fact that Hayek chooses as the relevant curve the one which is *touched* by the productivity curves implies that the dictator is supposed to follow the criterion of maximising the present value of the inputs. It is obvious that the value which the dictator places on the inputs must be derived from the value of the product. In the diagram the value of a unit of input invested at $O$ whether it ripens out at $L$ or $L_1$ must be $OH$, whereas the total value at the date $O_1$ of the inputs invested at $O_1$ and ripening out at $L_1$ must be $O_1H_1$.[1] A lower discount curve would give smaller values for the inputs and will therefore not be chosen. If we assume more than one kind of input, the productivity curves, while they will each touch one or other of the parallel discount curves, will not all have to touch the same one. This simply means that the present value of the different kinds of input will be different.

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[1] See the following footnote.

Up to this point Hayek's treatment of the problem is strictly analogous to Wicksell's treatment of the case of the individual entrepreneur. Wicksell assumes that for the entrepreneur the value of input and the productivity function is given. On this basis he proceeds to find the maximum rate of return and the investment period. Hayek, on the other hand, assumes the rate of return and the productivity function to be given, and this allows him to determine the period of investment and (although he does not stress this point) the maximum value of input. But just as Wicksell, when considering society as a whole, had to drop the assumption that the values of the inputs (factor prices) are given, so Hayek has to drop the assumption that the rate of return is given. Wicksell solves the problem for society as a whole with the aid of the datum " subsistence fund ". Hayek is aware that the subsistence fund cannot be considered as a datum determining the equilibrium. The crucial question then is what does he put in its place? We shall postpone the answer to this question until section 4.

(ii) Hayek emphasises only the fact that in equilibrium the rates of increase of the product divided by the product have to be the same in all lines of production. But this is not sufficient as a solution of the general equilibrium problem which confronts the dictator. Assuming two products to be produced in society with the help of only one kind of input, we evidently have the following eight unknowns in the problem.

1. The value of the total output of commodity (1). If we choose this commodity as the unit of account, the total value of the output is identical with the physical output.
2. The value of the total output of commodity (2).
3. The value of a unit of commodity (2) in terms of commodity (1).

4 and 5.
The number of units of input invested in the production of commodities (1) and (2) respectively.

6 and 7.
The investment periods of the inputs used in the production of commodities (1) and (2) respectively.

8. The value of a unit of input.

To determine these eight unknowns we have the following eight relationships:

1 and 2.
The value of commodity (1) must equal the value of input plus accumulated interest. Similarly for commodity (2).

3. The value of commodity (2) in terms of (1) must correspond to the ratio of the marginal utilities of the two commodities in the dictator's system of preference.

4 and 5.
The value of a unit of input in each line of production must equal its discounted marginal product.

6 and 7.
The marginal product due to a small lengthening of the period of investment divided by the product must, in each branch of production, equal the given interest rate.

8. All available inputs must be employed.

### (b) *The continuous input—point output case.*

This is the case treated by Böhm-Bawerk and described above on p. 303. Here the input function is known and is continuously variable. Our problem is first to find the difference in the contribution of an input when it is invested for two slightly different lengths of time. According to Hayek we are here confronted with new complications, the most important of which is the following. The product of the unit of input under consideration is here dependent not simply on the investment period of that unit of input, but also on the investment periods of all the other units which go into the process. In other words, the investment function of a unit of input is not independent of the investment functions of the other units.

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[1] A unit of input invested at $O$ and ripening out at $P_1$ will give a product of $P_1L_1$, the unit of input having a value of $OH$. At $O_1$ this unit will have accumulative interest, the total value of the investment now being $O_1H_1$. If a second process, employing the same kind of input, is started at $O_1$, and is to give the same output ($P_1L_1$) as the first process at $P_1$, it is clear that the value of the input invested at $O_1$ in this second process must be $O_1H_1$, which necessarily represents more than one unit of input since no interest has yet accumulated at $O_1$ in the second process. This explains the statement made previously in the text that the investment process starting at $O_1$ and ending at $L_1$ will require more input than the process starting at $O$ and ending at $L_1$.

This difficulty is not insurmountable. If we assume continuous variability of the process of production with respect to the investment periods of the various inputs used, it is legitimate to suppose that we can find out the addition to the output due to the lengthening of the investment time of any particular input on the assumption that the investment periods of the other units are given. This procedure can be followed for each kind of input. The principle is, of course, the same as that according to which we find the marginal product of a particular kind of labour or other factor. Again stationary equilibrium in this system is reached when the inputs have investment periods such that the rate of growth is everywhere equalised.

Just as before, this answer leaves some problems open. For a complete solution of the general equilibrium problem we should have to reformulate the equilibrium conditions that were given for the point input—point output case and add new ones for the new unknowns which enter the picture. The problem becomes very complicated. The best method of approaching it is probably to shift gradually from the point input—point output case to the continuous input—point output case by assuming that a second input is invested in each process at a point of time (not fixed by technical considerations) which lies between the beginning and the end of the process of production. We will assume that this second input is of a different kind from the first input. We then have five new unknowns.

1 and 2.
The amounts of the second kind of input invested in the production of commodities (1) and (2) respectively.
3 and 4.
The investment periods of this second kind of input in the two lines of production.
5. The value of the second kind of input.

We also have five new relationships.[1]

1. The whole of the amount available of the second kind of input must be employed.
2 and 3.
In each line of production, if we divide the marginal product which is due to a small lengthening of the period of investment of the second kind of input, by the product of this input, we must obtain the given interest rate.
4 and 5.
In both branches of production the discounted marginal product of the second kind of input must equal its value.

(4) THE DETERMINATION OF THE INTEREST RATE. What we have done so far is to give an analysis along Hayek's lines of the conditions necessary for stationary equilibrium under the assumption that the interest rate is known. The crucial question still remains: How is the interest rate itself determined? The subsistence fund which was used by Böhm-Bawerk and Wicksell for this purpose is rejected by Hayek. The value of this fund in a stationary state cannot be considered as a datum which helps to determine the interest rate. On the contrary, it is among the unknowns of general equilibrium analysis, just as are prices and quantities produced in the system. What takes the place of the subsistence fund in Hayek's analytical framework?

Hayek replaces the assumption that the value of the subsistence fund is a datum in a stationary state by the new assumptions that the concrete structure of capital goods is initially given as the result of an historical accident, and that the dictator knows all the different income streams which can be obtained from these capital goods. This structure of capital goods is not the one which will exist once stationary equilibrium is reached (if it is reached at all). On the contrary, it will change continuously during the process of approaching equilibrium. By assuming an accidental composition of capital goods Hayek avoids the objection that the " datum " which helps to explain the interest rate is itself determined by the same process which determines the interest rate. Since this new " datum " makes it impossible to assume a stationary state right from the beginning, Hayek replaces the hypothesis of a stationary state by the assumption that the dictator aims at keeping the income stream of society constant from a given moment onwards.

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[1] Some of the equilibrium conditions given for the case of one kind of input will have to be reformulated. Thus the value of the total output of commodity (1) will now be equal to the value of the first kind of input plus interest accumulated during its investment time, plus the value of the second kind of input plus interest accumulated during its investment time. Similarly for commodity (2).

By a constant income stream Hayek understands an income stream which may vary in composition but has to lie on the same *n*-dimensional indifference surface as envisaged by the dictator. The latter, starting out with the given structure of capital goods, tries to maximise the output stream and at the same time to keep it constant. He has to reach this maximised output stream at once, i.e., from the date onwards when he begins to carry out his plan. It is very difficult to visualise what this implies. The idea may be made clearer by a simple example modelled after Hayek's own somewhat more elaborate illustration. If, up to the date when the dictator's plan comes into effect, an input which was used for *current* production created a value of three but could create a value of five if it were invested for two years, and if another input which used to be invested for two years and created a value of five would, if used in current production create a value of four, it would be advantageous to " switch " the investment periods of the two inputs. The total income stream would then be greater and could be kept permanently on the higher level by constantly reinvesting these inputs in the same fashion. More complicated cases are impossible to think through. The general result which Hayek obtains is that it will be advantageous to rearrange the investment periods of the inputs if the rate of increase of the product due to the lengthening of the investment period of some units is greater than the rate of decrease due to the shortening of the investment periods of other units by the same amount. Or: " The rates of increase of the product which are due to the extension of the investment period by any given interval must be the same for all units of input ".

Such a rearrangement of the investment periods of inputs implies, as we indicated above, that the concrete structure of capital goods must change over time. Some of the capital goods existing in the beginning will not be replaced because it is unprofitable to replace them. The stock of others will increase. In the same way the composition of the constant output stream will not remain the same over time, nor will the relative values of the commodities making up this stream. We have to deal with a process of continuous change. The system will approach a stationary state, but, according to Hayek will, in principle, never reach it. Since the relative values of the commodities fluctuate during this process, the condition given above for a maximum income stream should read: The rates of increase of the *value* of the product which are due to the extension of the investment period by any given interval must be the same for all units of input, the values being measured in any one commodity which we may choose as our unit of account.

According to Hayek this statement means only that the rates of increase have to be the same for intervals starting and ending at the same date. It does *not* mean that they will be the same over intervals of equal length but beginning and terminating at different dates. As long as the stationary state is not reached we have different rates following each other. There is, however, a connection between the rates on investments of different length. For example, if $1 invested for one year grows to $1.05 during the year, and if an investment of $1 in the next year grows to $1.06, then $1 invested for the two years will have to grow to $1.05 x $1.06, i.e., $1.113. The interest rate will therefore be 11.3 per cent. over two years.

But all this shows only what the idea of a constant maximum income stream implies with respect to the equality or inequality of interest rates on investments of equal or unequal length. It does not answer the cardinal question what determines the *level* of the interest rates. Strangely enough there is very little about this in Hayek's book. The general idea of Hayek's solution seems to be that the interest rate is fixed by the productivity functions of inputs with respect to time, together with the assumption that the income stream is to be maximised and kept constant. The latter assumption imposes restrictions on the periods for which inputs can be invested and thus determines the point on the productivity curve where the product has to mature and hence the interest rate. " The factor which limits the possible extensions of the investment periods is that as one unit of input is invested for a longer period, the output stream at the earlier date is reduced and the value of the products maturing at this earlier date is consequently raised. This means that the value of the marginal products of units of input invested for that earlier date increases with the result that it becomes profitable to invest more for that date. We have postulated that the available input must be used in such a way that the resultant income stream will be of constant size, i.e., that every gap caused by investing some units of input for longer periods must be filled by investing other units of input for correspondingly shorter periods. Therefore the condition that all units of input must be invested in such a way that the ratio between the marginal rate of increase of the product and the size of the whole product is the same for all units of input also determines the period for which each of these units of input has to be invested " (p. 191).

This solution leaves some questions open which I am unable to answer satisfactorily. But before going into them we have to draw attention to Hayek's new approach to the problem. Whereas the analysis of Wicksell and Böhm-Bawerk was purely static, Hayek's approach is dynamic. One of his data, the concrete structure of capital goods, undergoes changes in the process of approaching a stationary equilibrium. Although each successive composition of the capital goods is, on Hayek's assumption, indirectly determined by the initial capital structure, the latter is not a datum in the sense in which the term is used in static theory. The " data " of static theory do not change in the process of reaching equilibrium whereas the capital structure does. What Hayek describes is a dynamic movement which never comes to an end, a movement in which each successive stage is determined by the preceding one. This, of course, is nothing but a step in the direction of period analysis. In principle Hayek has given up treating the problem of capital as a problem of static equilibrium although his analysis very often retains the characteristics of statics.

There are several questionable points in Hayek's solution:

(*a*) The first concerns the concept of keeping the income stream constant. Hayek supposes that the dictator directs the economy in such a way that all combinations of goods produced by society lie on the same *n*-dimensional indifference surface so that the income stream of society is kept constant. This indifference surface cannot, evidently, represent the personal pattern of wants of the dictator. Such statement as: " The level of total satisfaction has to be maintained constant " (p. 158), must refer to society as a whole. But the concept of an indifference surface as applied to society as a whole presupposes a sort of collective indifference pattern which has no definable meaning. It is not permissible to *assume* that the dictator knows the indifference pattern of society if the latter is logically undefinable. According to Hayek the relative values of the commodities change all through the process of approaching equilibrium. From this it follows that the distributive shares will also change. But if this is so, what sense does it make to say that in spite of such changes the psychic income stream for society as a whole remains the same?

(*b*) The long quotation given above reads as if the attempt to achieve a maximum constant income stream would lead to one single interest rate ruling in the system. A single interest rate, however, would only exist in a stationary economy which Hayek does not, and (in view of the nature of his data) cannot presuppose. The mere fact alone that the dictator aims at a maximum constant income stream does not produce constancy of the interest rate over time. This is admitted by Hayek in his discussion of the implications of a constant income stream. And again in his analysis of investment periods he states: " For all processes maturing at the same moment of time the ratio between the rate of increase of the product and the size of the product itself will have to be the same. This does not mean the ratios must also be equal for processes terminating at different moments. That is only true under stationary conditions " (p. 184). Taking the interest rate on a short investment (which may stand for the instantaneous rate of interest) as the " leading " rate, Hayek then admits that this rate will change over time and he gives a rule which connects the rates on the longer investments with the succession of shorter ones. But there is no attempt in the book to explain in detail what determines the succession of short rates. In the absence of such an explanation the rates on the longer investments are not determined either, during the process of approaching equilibrium. Hayek's general solution (quoted at length above) does not seem sufficient to tell us what the rate (or rates) of interest is (or are) going to be when we start out with a given structure of capital goods and postulate a maximum constant income stream over time. The condition " that all units of input (measured in value terms)[1] must be invested in such a way that the ratios between the marginal rate of increase and the size of the whole product is the same for all units of input " holds only in a stationary state, not during the process of approaching that state, when the rates of return on investments maturing at different dates are admittedly different. The condition therefore is not adequate to determine the interest rate during the process of continuous change which results from his own assumptions. To explain how this interest rate is determined under the latter should, however, have been his main task.

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[1] My insertion.

(*c*) As soon as we admit that there will be different interest rates for different investment periods, the relationship between interest rates and the productivity curves of inputs on which the explanation of the choice of the investment periods is based becomes questionable. According to Hayek the ratio between the rate of increase of the product and the size of the product will have to be the same for all processes maturing at the same moment of time. This is supposed to be so even before stationary equilibrium is reached. In conformity with this idea the productivity curves (drawn in Diagram 2) of all processes ending at the same date touch the same discount curve, no matter when they start. On the other hand, it is admitted that during the process of approaching equilibrium interest rates on investments of different lengths will be different even if the investment periods end at the same date. If, to quote the example above, the interest rate on an investment for the first year is five per cent. and for the second six per cent., then the rate on a two-years' investment will have to be 11.3 per cent., which corresponds to an " average " rate *per unit of time* (a year) of approximately 5.5 per cent. How can we say that investments starting at different dates, but terminating at the same date, will touch the same discount curve if a two-years' investment gives 5.5 per cent. and a one-year investment terminating at the same date as the two-years' investment gives six per cent? We should have to make the quite arbitrary assumption that what " really " happens is that the longer investment grows in the first unit period at five per cent. and in the second at six per cent. Hayek's approach would impose the task of solving the whole problem of the structure of interest rates and its connection with the productivity functions of inputs.

Summarising we may say: Hayek's new approach substitutes the concrete structure of capital goods for Wicksell's subsistence fund and the concept of a constant maximum income stream for the concept of a stationary state. He avoids in this way the circular reasoning which results from the fact that the subsistence fund (a value concept) is itself determined by (among other things) the interest rate. But Hayek's solution is not entirely satisfactory. His approach leads him away from statics to an analysis of a process of continuous change, in which the income stream as a whole remains constant, but the concrete structure of capital goods, as well as the composition of the output stream, changes. All through this process the interest rates fluctuate. There is no satisfactory explanation of the movement of interest rates over time nor of the structure[1] of the rates at a given moment of time.[2]

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[1] In the last part of his book Hayek explains the structure of rates by the influence of the monetary factor. But as he admits that such a structure would exist even in the absence of monetary influences, the explanation by reference to the latter is not sufficient.

[2] I do not go into Hayek's treatment of the problem of interest in a competitive society. As far as I can discover, it throws no new light on the determination of the interest. Hayek's procedure is quite unusual. He assumes again a given structure of capital goods, the latter now being in the hands of individuals. The services of these goods can be invested for different lengths of time. Hayek assumes first that the owners of ready consumers' goods, and of resources which can be turned into consumers' goods within a unit period, say, a month, consume them entirely in the month. The resources which cannot be turned into consumers' goods within a month would then be valueless. He then supposes that more and more of these ready consumers' goods will be invested, instead of being consumed during the month, i.e., they will be exchanged for resources which produce consumers' goods at some future time. The result is that the value of the resources will rise and the rate of return on " investments " will fall. In equilibrium (which, however, is not likely to be reached) " the values of the future consumers' goods must exceed their costs (in terms of present consumers' goods) by an amount which bears a uniform relationship to the time which will elapse before the future consumers' goods become available " (p. 264). The rate of return on investments is thus dependent on the price relationship of capital goods and consumers' goods. This relationship in turn is determined by the demand for future consumers' goods in terms of present consumers' goods (the future consumers' goods having in the present the form of services of permanent and non-permanent resources).

In this analysis the productivity functions of inputs with respect to time which play such an important role in the earlier chapters are not mentioned at all, although it is not impossible to work them into this kind of approach. As it stands the analysis of the interest rate in a competitive society does not seem to be very closely connected with the fundamental analysis given on the assumption that a dictator directs the economy.

# A Comment

By F. A. v. Hayek

Space and time being at the moment in equally short supply, it is impossible to respond at once to Dr. Lutz' careful analysis of the treatment of interest in my *Pure Theory of Capital* with the considered reply it deserves. But I wish at least to suggest that Dr. Lutz' difficulties may be due to his stopping his analysis short of the sections of my book which seem to me to contain at least the germ of a solution. The assumption of a "constant income stream" was, as I tried to explain (p. 216), adopted merely provisionally as an expository device in the early parts of the book, to be dropped later. A complete solution can emerge only after this assumption is given up and replaced by an analysis of the preferences for increments of income at different dates. Such an analysis is sketched in chapters XVII and XVIII which Dr. Lutz excludes from consideration.

In so far as Dr. Lutz' contention is that these chapters are inadequate or insufficiently integrated into the exposition, I readily admit the justness of this criticism. The fact is that once one pursues the time-preference analysis, as would be necessary for the purpose, beyond the simplest comparisons between two points of time, to that of comparisons between all the different future dates for which the planner has to provide, the task becomes so absurdly complicated that I abandoned it in alarm. Such a detailed analysis of the structure of time preferences, analogous to that which I had attempted with respect to the productivity element, seemed to me to belong to the further elaborations where the result was not likely to repay the pains of the author or the reader. I regret if the sketch I have attempted does not sufficiently indicate the principle of the solution, and particularly if the brevity of the treatment, and one or two unguarded generalisations, give the impression that I attach to the time-preference aspect less importance than is probably due to it. It is in this respect more than in others that I am now most conscious that my exposition requires revision.

# Book Reviews

*Studies in the National Income 1924–38.* Edited by A. L. Bowley. Economic and Social Studies I. National Institute of Economic and Social Research. Cambridge University Press. 1942. ix + 255 pp.

This volume is the first of the series of Economic and Social Studies prepared under the auspices of the National Institute of Economic and Social Research. Other publications have followed. The series is greatly to be welcomed by economists and especially by British economists who may hope that the works issued by their own National Institute may come to enjoy a prestige comparable with that of its American counterpart, the National Bureau of Economic Research.

The director of the present investigation was Professor Bowley, who was assisted in his labours by Mr. Booker, Dr. Elsas, Dr. Neumann and Miss Dessauer. Some work of Miss Marley and Mr. Campion[1] is reproduced and the latter was consulted in the preparation of Chapter II.

The book consists of four chapters and a bibliography. The chapters are entitled: The definition of national income, Some constituents of the national income, The census of production, and Price movements: index of real income.

The first chapter is mainly concerned with the answers given by a number of writers on national income to a questionnaire on definitions, a discussion of the replies, and a statement of the treatment adopted in certain British official returns on income and output, notably those of the Board of Inland Revenue and the Census of Production. It is not always clear whether the questionnaire has actually been answered by the authority or whether his treatment has been deduced from his published work. Also the replies are not always dated, so that in these cases it must be assumed that they were derived from the latest works, or, in the case of direct replies, in the period 1938–39. It would be useful to have a more exact account of the date of the replies since the subject is developing and it is possible in some cases that the views expressed are no longer held. Finally, the discussion is piecemeal rather than from the point of view of a comprehensive conceptual system, and attention is concentrated on the treatment of various elements in income without much discussion of what the alternatives imply on the side of output or expenditure. Nevertheless, despite certain vaguenesses of expression, this section is a useful summary of the views of a large number of writers on many points of definition.

Much of Chapter II will be familiar to readers of the *Statistical Journal*, since parts of the enquiry have already appeared there. Part 1 on some constituents of the national income is reproduced with minor additions and corrections from an address by Professor Bowley[2]; Part 2 on changes in salaries in Great Britain is a reprint of the work of Miss Marley and Mr. Campion referred to above; Part 3 is on income from agriculture; Part 4 deals with the number of income earners; and there are two final notes on the distribution of incomes among persons, and the valuation of stocks and partly finished goods.[3]

This chapter contains a great deal of valuable statistical information on the constituents of income estimates. It raises the interesting question of margins of error since these are affixed to several of the calculations. On p. 80, for example, such margins are given in respect of the main constituents of income for 1931-32. The individual margins are compounded by taking rather more than the square root of the sum of their squares, i.e. on the assumption that the errors in the components are not quite independent but slightly positively correlated. This is surprising since it is stated in the text that if anything there is probably a slight negative correlation among the errors. However this may be, the margin of error of the major constituents is put at £50 million, a result which seems to imply a quite remarkable confidence in the data. The main factor which makes possible this result is that no margin of error is given to income assessed to tax. This could only be justified by agreeing to accept without adjustment such figures as appear in the Reports of the Board of Inland Revenue. But this cannot plausibly be done, since apart from the question of evasion and overassessment which for some reason is only allowed for in 1924, there are additional difficulties, such as the precise date to which the published assessments should be taken to refer, the way in which current losses and past losses are brought into account in the assessments, and the varying rates granted in respect of additional allowances for wear and tear. In view of such considerations it is hard to see how a zero margin of error can be sustained.

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[1] *J.R.S.S.*, Pt. IV, 1940. Changes in Salaries in Great Britain, 1924–1939.

[2] *J.R.S.S.*, Pt. IV, 1940, pp. 491–518.

[3] Since this was written the following articles on the same subject have appeared in Economica:
A. L. Bowley: "National Income in America and in the United Kingdom," August, 1942, pp. 233–236.
T. Barna: "Valuation of Stocks and National Income," November, 1942, pp. 349–358.
A. L. Bowley: "Note on Mr. Barna's Valuation of Stocks," February, 1943, pp. 64–65.
A treatment of stock estimates not from the point of view of theory but from that of the contribution made to the discrepancy between Mr. Barger's estimates of income and expenditure in the U.S.A. was given by the reviewer in the *Economic Journal* April, 1943, pp. 71–73.

In view of the level of some of the other margins—the one for wages is approximately 3 per cent. of the wages bill—it is to be supposed that the margins of error are roughly equivalent to standard errors and not to limits between which the true value is supposed certainly to lie. Unfortunately, however, the precise nature of these margins is not stated. In comparing these estimates for 1924 with others derived by the output method, a margin of £100 million is given.

The output estimate used for this comparison is taken over with certain modifications from Flux. In this case, however, the margins of error for such elements as are given them are added up (or practically so) which is equivalent to assuming that the errors in the elements are perfectly correlated. This is not a very likely assumption, so that if the constituent margins are granted the error in the total is certainly too high. Again, the nature of the margins is undefined, and in the case of this comparison recognition must be given to the fact that the margins are assessed by different investigators.

The components used in the comparison of independent estimates of income and output in 1924 are set out in a way which makes the logical basis of the calculation rather obscure. The main reasons for this are two. First, the component elements partition the totals into units which arise from the limitations of the statistical data rather than into those which would follow most naturally from economic considerations. Second, no tables are given showing the constituents of other accounts in the economy, notably the balance of payments and the accounts of public authorities.

The basic national income concept used is taken from Chapter II and contains, in addition to wages, salaries, profit, interest and rent earned in productive activity, also pensions, employers' contributions and national debt interest. The first two of these catagories are, I think, regarded as part of the earnings, imputed or actual, of factors of production, whilst the last, national debt interest, is regarded as a transfer. The national income figure also includes property income due to foreigners as well as income derived from British property overseas. For 1924 this total is estimated as £4,273 million, from which £600 million of direct taxes and rates is subtracted before comparison is made with output.

The output estimate is based essentially on the concept of output for home consumption, i.e. total production *plus* the services necessary to transport and distribute these goods *plus* all other services including the services of houses, which are consumed at home but *excluding* certain services rendered by home factors (employed, for example, in the shipping industry) to foreigners, and services rendered by public authorities. Professor Bowley adjusts Flux's original figures by the addition of the value of services rendered by persons working on their own account and by excluding such government goods and services as appeared in Flux's calculation.

To these figures it would seem natural to add shipping earnings, commissions, etc., earned on foreign account *less* the value of such services appearing in British 'net output' as were provided by foreigners, and also the output of British property abroad. It is not necessary to deduct the output of foreign capital here since this sum is included on the income side. Instead, however, Flux preferred to include the items on the opposite side of the account, namely the excess of imports over exports *plus* overseas investment. The latter is put at £50 million, the figure given by Flux, although the estimate now given (e.g. in the *Statistical Abstract*) is £72 million.

It appears, therefore, that the treatment adopted is broadly consistent though somewhat tortuous. Would it not have been more convenient for the reader to add on the items directly required rather than their counterpart on the opposite side of a balancing account? This would not have been difficult since the necessary information is provided in the official balance of payments estimates. But it may have been thought desirable to depart as little as possible from Flux's original presentation.

Again, the items taken from the public accounts are at first sight difficult to follow. Income includes national debt interest and certain pensions which are not paid directly out of business receipts but excludes direct taxes and rates. Output, on the other hand, excludes goods and services directly provided by government agencies (as opposed to those which are purchased from private business) but includes customs and excise duties. Very broadly speaking, this is also consistent in principle since indirect taxes *less* government goods and services equal transfer payments *less* direct taxes. But the actual treatment given appears to ignore unassessed government income from property, borrowing and subsidies, which may have been small, and also goods and services purchased from private industry and transfer payments other than those mentioned above. In the absence of an account showing the receipts and expenses of public authorities, it is impossible to say how far these elements cancelled out in 1924.

Depreciation is deducted from output by taking Flux's original £305 million. It is not clear how far this covers not only depreciation allowed but also repair and maintenance charged to current account. In principle, the latter should also be deducted since it is excluded from profits and rent in the income total but included in net output in the production estimates.

It is clear that the final figures compared do not accord with any commonly used concept of income and output. This however may be due to a concern with the independence of the two sets of figures, rather than with the precise total to which they add up. It would appear that there may still be differences of scope which have not been taken into account especially in the field of the public accounts.

The section on income from agriculture makes an interesting comparison between the results derived from the income and output approaches. The results give quantitative expression to the well-known fact that over this period the income approach leads to deficient totals for income generated in agriculture mainly on account of the conventional method of assessment adopted under Schedule B. Yet, despite all this, the appropriate addition is not made to the national income estimates, though as mentioned above the figures derived from the Board of Inland Revenue are not provided with any margin of error.

Chapter III contains estimates of the change in the volume of output between the censuses of 1924, 1930 and 1935.[1] Reference is made to the work of census authorities, Dr. Rhodes and Mr. Devons, but no attempt is made to bring in or rework the estimates of Messrs. Tolles and Douglas[2] on the variation in output between 1907 and 1924.

An attempt is made on pp. 140–141 to obtain an estimate of variations in the cost of materials used. If $p_0\, q_0$ equals the gross value of output in period 0 and $p'_0\, q'_0$ equals the net value of output in period 0, then the materials price index with period 0 as base, $P_{01}$, is given by

$$P_{01} = \frac{\Sigma(p_1\, q_0 - p'_1\, q'_0)}{\Sigma(p_0\, q_0 - p'_0\, q'_0)}$$

This result makes use of differences between gross and net values and would appear therefore to be affected by the grouping of industries and in general to involve some duplication. A comparison with wholesale price series indicates close agreement between 1930 and 1935, but a considerable discrepancy between 1924 and 1930.

Section 4 of this chapter goes over the ground covered by some earlier work of the reviewer.[3] The advantage of expressing output in terms of two variables; employment, so as to allow for variations in short period returns, and a trend, so as to allow for technical improvement and the growth of capital equipment, appears to have been misunderstood. The calculation given in this section does not take into account variations due to short period changes in employment, but takes one rate of increase in output per head between 1924 and 1930 and another, nearly twice as big, between 1930 and 1935. The resulting numbers show remarkably little fall in the slump and leave one in some doubt about the significance of any relationship found.[4]

The London and Cambridge annual index is stated to be the most consistent short period measure of output (correctly so far as its movement between the three census years is concerned), but its more recent movement and especially the large fall between 1937 and 1938 would appear to be at variance with Professor Bowley's income data. For it is reasonable to suppose that the fall in the output of material goods in a recession will not be offset by a rise in the output of services, and if this is granted no plausible price index could be found which would deflate the estimates of income between 1937 and 1938 so as to agree with the apparent fall in output. Short period measures of output in this country before the war are not a matter for complacency, and it seems an unnecessary counsel of despair to shift the blame on to the inherent difficulties of the subject.

The theoretical part of Chapter IV on the measurement of real income is reworked, with some change of notation, from a paper read to the Manchester Statistical Society in 1939. The main feature of this part of the work is the treatment of goods and services purchased by the government and the taxation necessary to pay for this expenditure. In dealing first with an economy in which government purchases are financed by direct taxation only, it is shown that an absurdity is reached if the prices of government goods and services are put at zero. This treatment is adopted first on the ground that the goods and services are distributed free and are not, therefore, paid for directly by the private sector of the economy. But it is abandoned in favour of a treatment which regards the government as the agent of the community in the purchase of certain common services for which purposes direct taxation is levied. A market price is imputed to these goods and services equal to their cost and the measure of change in real income is expressed as an aggregative index of the quantity of privately purchased goods and services and those purchased *via* government agencies, each weighted by market prices.

The case where government purchases are financed by indirect taxes is treated differently. Considering the case where government purchases are equal to the yield of indirect taxation, it is argued that, since the weights of the index are market prices and not factor costs, the value of government purchases is already included in the market value of the goods and services purchased directly by the private sector of the economy. On this argument it would be double counting to include an amount equal to indirect taxation again as the imputed price of government goods and services. Accordingly in this case government goods and services are imputed a zero price, while privately purchased goods and services are valued at market prices. The index of real income is therefore given as an aggregate of the quantities of privately purchased goods and services weighted by market prices.

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[1] For a commentary on some of the methods used in this chapter, see H. Leak, *The Volume of Industrial Production*, Part I, 1943, pp. 60-63.

[2] "A Measurement of British Industrial Production," *The Journal of Political Economy*, February, 1930, pp. 1–28.

[3] See *Economic Journal*, September, 1939, pp. 476–485.

[4] The equation used in the study referred to in the preceding footnote was linear in $E$ and $t$. Though unimportant in the present context in view of the comparatively minor fluctuations in the variables, it is preferable in principle to use an equation of the form $O = aE^b e^{ct}$ where $O$ is output, $E$ is employment, or, better, man-hours worked, $t$ is time, $e$ is the base of natural logarithms, and $a$, $b$ and $c$ are constants. Data for the United States provided by the Department of Commerce and the National Bureau of Economic Research make possible the construction of indices of output of, and man-hours worked in, American manufacturing industry over the period 1899 to 1942. The variations in output over the whole period can be expressed with a high degree of accuracy by the above equation with $b = 0.90$ and $c = 0.028$. Not only is the fit good but the bunches in the set ($\log O$, $\log E$, $t$) are very tight. Using this form of equation, the value of $c$ for this country from the data referred to in the preceding footnote works out at 0.027. In the latter case, however, $E$ relates to employment and not to man-hours worked.

This does not seem to me to accord with ordinary notions of real income. For suppose a simple economy which in peace-time produces only consumption goods but in time of war pays for government payments to the armed forces and purchases of stores and equipment by means of an indirect tax on consumption goods. In the absence of unemployed resources, increase in productivity or some such factor, resources could only be freed for government use by a cut in consumption which would be assisted by the new taxation. But it would surely be unreasonable to regard the falling quantity of consumption as a measure of real income, yet this is what Professor Bowley's argument appears to involve.

A way out of this difficulty is to recognise that there are two different sorts of indices of real income that may be constructed which in general will differ from each other. First we may use market prices as weights, in which case we shall obtain information about the situation from the consumers' point of view and our estimate of quantity change may be called an index of real expenditure. On the other hand, we may compile an index for which the weights are not market prices but factor costs, i.e. market prices *plus* subsidies *less* indirect taxes. This index will provide information from the producers' point of view and may be called an index of real output. An identical measure could be obtained from output data by weighting the quantity of each article processed in the periods compared by its process margin, i.e. the net output of that process in the period.

To my mind it is not correct to regard the expenditure index, which includes government goods and services at the market prices paid for them by government agencies, as involving double counting. For each article of final expenditure is weighted by its market price and thus takes account of the price structure which faces purchasers of final goods and services. As mentioned above, this index will not be the same as the output index since the prices which act as weights are relatively different unless all final purchases bear a uniform *ad valorem* tax. Which of these two measures, expenditure or output, should be described as reflecting changes in real income I am not sure, though I should favour the former. But I do not think that an index of direct purchases at market prices made by the private sector of the economy alone can reasonably be described as an index of real income in circumstances in which a considerable part of the national output is routed through government agencies.

One further point may be added. It might be argued that only a part of government purchases of goods and services should be regarded as final purchases. This would clearly involve changes in the treatment of income and expenditure but is not I think relevant to the present argument.

At a later stage an attempt is made to work transfer payments into the expression for real income. To avoid ambiguity it is found necessary to apportion the expenses of transfer payments between direct and indirect taxation. This procedure seems to me without significance. Transfer payments form, by definition, no part of the output of goods and services either at factor cost or market prices and consequently would seem to have no place in an index of real income. The concept of the net national income plus transfer payments which may be called disposable income is a useful concept in analysing variations in demand because it brings together all payments which accrue as income irrespective of whether or not they have been earned in producing current output or have been transferred without a service being rendered. But such a concept cannot be deflated in the sense in which output can. This is not to say that it is inappropriate to divide by a price index of, say, consumers' expenditure so as to express it in terms of its purchasing power over consumers' goods and services.

We come finally to the bibliography. This is a disappointing piece of work. It is immensely long, yet substantial works on many of the subjects that it covers are omitted. At the same time there are many references to old and obscure works and to the semi-learned periodical literature of the last century. In short, it is neither complete nor an *Index Expurgatorius*. Further, the arrangement of the contents of its various sections is difficult to understand and it contains an inordinate number of misarrangements, errors and slips from the last of which the book as a whole is unfortunately by no means free.

It is not possible within any reasonable limits to touch upon all the points of interest which arise in this book. It contains information which is essential to all working in this branch of economic and statistical analysis. It is not an easy book to read. Its authors do not appear to have been at great pains to smooth their reader's path by providing a clear and orderly presentation. Rather they have flung their wares before him and bidden him unravel them to discover which are to his liking.

RICHARD STONE.

*Social Goals and Economic Institutions.* By FRANK D. GRAHAM. Princeton University Press. 1942. xxii, 273. $3.00 20s.

There will no doubt be a certain raising of eyebrows at an economic treatise which the author himself describes as " a confession of faith ". But if these are not the days of faith then there seems little meaning in our present miseries. Let it be said at once, that this book is not only a reminder of the important contribution which confessions of faith by economists have made to the solution of social problems in the past, but also a convincing testimony to the present powers of its author in this field. It is a long time since such a breath of intellectual fresh air blew from the other side of the Atlantic.

The author boldly lays down as the essential social goal " the release and enlargement of the power of men as a whole " over the rest of creation. Power in this sense is a good but the coercion of men by one another is an evil. The social system should be devised so as to enlarge the powers of men without leading them to destroy one another's freedom. In the economic sphere the conditions of the development of productive power and the maintenance of freedom are the enforcement of a system of private property and free competition.

The argument so far is not essentially new though it is stated by Professor Graham with great freshness and vigour. The real contribution of the book lies in its attempt to think through the arguments of the classical economists once more in the light of modern conditions, whilst accepting as an article of faith their social and political premises. " Go back to Bentham," says Professor Graham to us all, in effect, " and start thinking again." In the present work this process sometimes gives unexpected results, which, at the risk of misrepresentation, may be very shortly summarized here :—

(1) The strict limitation of inheritance in order to prevent gross violation of the righ to equality of opportunity.

(2) The reform of company law as part of a drive against monopolistic tendencies in business. The suggestions follow the lines of those made by Simons,[1] including limitation on the size of companies and prohibition of inter-company stock holdings, together with a proposal that taxation of new independent and small concerns should be entirely remitted in order to stimulate enterprise.

(3) The restriction of Government indebtedness even when incurred to pay for war and relieve unemployment. " Nothing . . . . that can be accomplished by Government debt is impossible of attainment through appropriately distributed taxes."

(4) The abolition of progressive taxation, which, if the principle of equality of opportunity obtains, is without warrant. The substitution of strictly proportionate taxation.

(5) The introduction of " commodity reserve " currency. This scheme, due originally to B. Graham, involves the issue of legal tender currency against the deposit of warehouse receipts for collections of commodities of fixed composition. The currency would be redeemable in warehouse receipts giving title to the same quantities of commodities as led to its issue. The object would be the stabilisation of business activity by the injection of purchasing power at an early stage in a depression (goods being exchanged for money) and the withdrawal of purchasing power at the early stage in a boom (money being exchanged for goods). " Commodity reserve " money has not yet been fully discussed in this country where the present preoccupation is rather with the particular problems of post-war unemployment than with the general problem of cyclical fluctuations in trade caused by periodical deficiencies of aggregate purchasing power. Though the latter might be successfully dealt with by commodity reserve money it may be doubted whether the patches of " conversion " unemployment which may afflict the armament producing centres and the war occupations in the post-war period will yield to treatment quite so easily unless the rate of injection of new money reaches inflationary levels.

Apart from the merits of the particular proposals in Professor Graham's programme, there must be many who have considerably more than a sneaking sympathy with the political philosophy behind them, i.e. that progress demands not equality of wealth but equality of opportunity, that the continued functioning of democracy cannot be guaranteed while the economic sea is dominated by monopolies tolerated or even encouraged by Government, and that current proposals for mitigating unemployment are too often politically dangerous as well as being economically dubious. There must also be many British economists, even those who, like the present reviewer, have not yet read the 43 monographs and 40 volumes of Hearings of the U.S. Temporary National Economic Committee on the subject, to whom these discussions appear somewhat old-fashioned, however hotly the great debate may rage in the United States at present. Any such may well be recommended to ponder the dictum of Burke that " this fierce spirit of liberty is stronger in the English colonies probably than in any other people of the earth ", and then to settle down to *Social Goals and Economic Institutions*.

S. J. BASTER.

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*The Economic Novel in America*. WALTER FULLER TAYLOR. University of North Carolina Press. Oxford University Press. 1942. xi + 378 pp. $4.00. 24/-.

Most modern literary criticism makes certain assumptions concerning the relations between literature and society. It is for example now an axiom that a preoccupation with social problems improves the quality of any piece of writing and that the more significant social trends of any period can clearly be deduced from literature, particularly from the work of the greatest writers.

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[1] A positive programme for *laissez faire*. H. C. Simons. Chicago, 1934.

In this view the writer is passive, he merely reflects or possibly 'interprets' certain social forces. The whole attitude is admirably summarised by John Dos Passos when he says "I cannot see how even the most immortal writer is more than the best possible type of moving picture machine contrived to focus the present movements on the screen of the future." This doctrine is of importance in many fields of intellectual effort; its importance for literary criticism is obvious, the sociologist is intended in literature as a sociological phenomena and literature is a part and a large part of the material of the historian, especially of the social historian. Mr. Taylor has for the first time subjected the theory to examination in the light of actual facts. He has written a careful and detailed study of all those novels appearing in America between 1820 and 1900 which dealt with social or economic themes. Such a study could be undertaken either from the point of view of the literary critic or of the sociologist. Mr. Taylor writes as a literary critic and an historian of ideas and not as a sociologist.

This period known in American literary history as the "gilded age" in Mark Twain's famous phrase, has been a particular target for proletarian and sociological attack. Ever since the end of the last war a series of studies have appeared both of the period in general and of individual authors explaining the weaknesses of this literature by the growth of industrialism and by the fact that the novelists accepted capitalist industrialism and produced no social criticism which could be compared to that of Morris and Carlyle in England. Van Wych Brookes put the case in the first and probably the best of these books *The Ordeal of Mark Twain* in 1920. The whole attitude, of course, has been powerfully re-inforced by the proletarian school of criticism. Mr. Taylor has set himself to examine this thesis and has shown quite conclusively that the American Novel cannot be accused of indifference to social questions or of any lack of sympathy with the working class. William Dean Howells, Hamlin Garland and Edward Bellamy and a host of lesser novelists were socialists and Frank Norris certainly showed no tenderness for the entrepreneur. What is less obvious and what Mr. Taylor seems doubtful about himself is how far the social contents of these books added to their literary quality, and economists may well ask themselves whether the social and economic views of the authors showed any real understanding of economic problems. With regard to the literary quality Mr. Taylor says "that the plain simple fact is the specialised subject-matter of the economic novels has neither helped nor harmed them as works of art, and that they are no whit better or worse than are novels in general." He might have added that the one novelist of the period who would be considered by everyone, even by proletarian critics, to be of the greatest literary significance is the novelist who concentrated on the life of the rich: Henry James wrote no economic novel.

Nor is it by any means clear that, if their authors had no interest in social problems, the books would have been really different in any important sense. Mr. Taylor says "to many of the art qualities of the economic novel . . . . the social background hardly enters at all, and this is especially true of all that pertains to the author's method of fiction, as distinguished from their materials . . . quite evidently, the style and method of the economic novel are, in all matters except those evidently dependent on the author's personality, simply the outcome of contemporary usage." This is particularly important from the point of view of Marxist criticism. "It is not the consciousness of men that determines their existence, but on the contrary their social existence determines their consciousness." This, if it means anything, surely means that changes in technique automatically produce a change in men's consciousness which is reproduced in literature. The word reflection invariably used by Marxists expresses exactly what they mean, a reflection is automatically produced by a certain set of physical circumstances. If the theory is correct all novels, including the most popular, should surely show a change in style and method as well as subject, a change which, however much it differed in quality, would be a change in the same direction. Of this there is absolutely no trace although the period is the one in which the technical foundations of American civilisation were entirely changed by the advent of machinery and of modern industrial organisation. Some of the novelists, of which the chief was Mark Twain, welcomed mechanisation whole-heartedly, others were repelled by it, but it made no difference to their own methods and even the content of the novel was less fundamentally affected, than might be supposed at first sight. It is true that the background was changed, and new characters were produced to correspond with the new characters which industrial life produced. But as Mr. Taylor points out, "the established stories of fiction, based as they are on a few, central, permanent human relationships have a way of perpetuating themselves from age to age." For example the story "in which some sensitive individual falls victim to the unjust institutions of society" could be used to attack the institution of slavery in *Uncle Tom's Cabin* and the institution of capitalism in a novel by Howells. From the purely literary point of view a most moving story could no doubt be produced on these lines, and a real hatred of any given institution would perhaps give a greater intensity to the novel. But from the point of view of social criticism it seems doubtful whether such novels are really very valuable. *Uncle Tom's Cabin* probably helped the abolitionist propaganda, and slavery was certainly an institution that required to be destroyed, but it would be quite easy to construct a plausible tale about the sufferings of an individual under any social system; certainly a revolution can be attacked from this point of view and the only answer a revolutionary can give, is that it is all a question of the ultimate result in terms of the happiness of the majority. But it is exactly the question of numbers with which novels and plays cannot deal and which seem to me to vitiate any claim which the novel or play may have to be a vehicle of serious social criticism. It seems to me very doubtful whether the economic novel really "did provide all necessary means whereby the society of the newly born machine age might be made known to itself." How far can an author really give a picture of the age even if he consciously tries to do so? No doubt novels do give a fairly accurate picture of the manners of any period, although it is useful to remember Lady Mary Montagu's criticism of Richardson, that he knew nothing of the manners of the society which he portrayed.

The extent to which novelists accurately reproduce manners largely depends on literary conventions. Of the six modern novelists whom Mrs. Lewis considers are worth reading, Virginia Woolf, I. Compton Burnett, T. E. Powys, E. M. Forster and James Joyce, only Virginia Woolf and E. M. Forster can be said to reproduce actual manners and conversation with any fidelity. As to those deep-seated social trends which novelists are supposed to mirror so exactly the very nature of the novel seems to work against a faithful rendering of social problems. A novelist will naturally seize on the more obvious and more spectacular social phenomena. For example, Frank Norris in *The Pit* took an actual event, the wheat "corner" engineered by Joseph Lister. But though exciting and eminently suitable material for a novel, cornering commodities is not really the most significant aspect of the workings of commodity markets but a rare and unusual incident and taking the whole system into account is entirely trivial in its effects; again if we take the ordinary run of novelist their fondness for strikes would surely lead an historian who had no other information to suppose that the relations between capital and labour in America were particularly bad and likely to get progressively worse. As we know the contrary is true; this the contemporaty novel conceals because the strike is an eminently dramatic spectacle, while the process of production which so greatly raised the standard of living in America was largely concealed. From this point of view the only writer who had any real apprehension of the machine age seems to have been Mark Twain. Another way the novel can obscure the contemporary scene is in the interpretation of society in the light of general theory, for example that of the struggle for existence. This theory so dominant in the nineteenth century undoubtedly tends to over-rate the ruthlessness of competition and led them to such exaggerations as Robert Herrick puts into the mouth of his Doctor, "a brutal game, this business success—a good deal worse than war, where you line up in the open at least". Competition and war from any scientific point of view are social institutions of the most widely different character and the metaphor of war does not really lead to any deeper understanding of the nature of competition.

Mr. Taylor's book has certainly removed prejudice and misconception from one literary period and it incidentally contains a great deal of information of the utmost value both to the literary critic and the historian of ideas.

D. SPEARMAN.

*Victory Is Not Enough! The Strategy for a Lasting Peace.* By EGON RANSHOFEN-WERTHEIMER. W. W. Norton and Company Inc., New York. 1942. 322 pp. $3.00.

The main merit of the author's contribution to the treatment of post-war problems is that he rather skilfully disposes of many popular prejudices and errors. On the other hand, Mr. Ranshofen-Wertheimer does not attach proper importance to the economic factors.

The author advocates European unity, a European Confederation. He believes that the major conditions which in 1930 rendered the realisation of the Briand project difficult have disappeared or are in process of disappearing. But this is unfortunately true only so far as the events of 1938-40 have shown most of the European countries the dangers of their separation and the impossibility of protecting themselves against aggression. (To the intelligent European statesmen—they were, of course, not very numerous—this was not a new illumination; they were long since fully aware of the lamentable impotence of their nations.) But the main problem is whether those economic factors which in the years between the two World Wars pushed every European nation into a permanent economic war against all other nations have disappeared too. As long as economic nationalism prevails and every nation is eager, in the economic sphere, to inflict as much harm as possible on all aliens, there cannot be any question of political unity and co-operation. Can we expect that the European nations will abandon trade walls, migration barriers, foreign exchange control and all the other measures of mutual economic warfare? This is the main problem.

The author under-rates the role played by economic nationalism in bringing about the present war and consequently the obstacles which economic nationalism puts in the way of European unification. The hard fact is that all nations are convinced that protectionism is indispensable for safeguarding their material prosperity. Let us abstract from the very delicate matter of migration barriers. But if the citizens of the comparatively overpopulated countries are prevented from exporting their products, they will not consider the European Confederation as a blessing but as a method of perpetuating existing economic inequalities. The Ruritanian worker will never acquiesce in a state of affairs which makes it impossible to reduce, to some extent at least, the existing inequality of wage rates between his own country and other countries. He does not care for the vested interests of labour of the comparatively under-populated countries. In his eyes the workers of Atlantis enjoy an unfair privilege. He aims at an abolition of this privilege and at a fairer distribution of income among the workers of various countries.

Mr. Milan Hodza, Prime Minister of Czechoslovakia from 1935 to 1938, has suggested a Federation in Central Europe. The present writer, for many years, has recommended an East European Union which should unite all nations between the Eastern borders of Germany and Italy and the Western borders of Russia. Both projects would not bring about European unity and would not solve Europe's economic problems. They would only safeguard the political independence of these small nations against future aggression on the part of their powerful neighbours. But even such an union could never come into existence if the customs walls between their members were to be maintained. It is usual to hold up Switzerland and the United States of America as models for the suggested European Federation. However, people forget that there are neither trade nor migration barriers within these federal unions. They would have been disintegrated long since if it were otherwise.

The main political problem of our day is economic nationalism. The fate of Western civilisation will not be decided by diplomats at the peace conference, but by the outcome of the discussion *free trade versus protection.* If the neo-Mercantilists prevail against the free-traders, there cannot be any question of lasting peace. No sophisticated union-project and no diplomatic makeshifts can make peace durable in an environment of economic warfare. As long as people disparage every free-trader as a reactionary " orthodox " and indulge in the superstition that conventions and the work of an international bureaucracy can abolish war, there is no hope for a more reasonable world order.

Mr. Ranshofen-Wertheimer was, for many years, a member of the League's secretariat. The experience acquired in this capacity gives his book a cautious realism. He does not share the optimistic illusions common to many pacifists. He provides the reader with a lot of information about unknown or unduly neglected facts and problems. These virtues more than outweigh the insufficient treatment of the economic problems involved. They make his volume one of the outstanding contributions to the main problem of our day.

LUDWIG VON MISES.

*The British War Economy, 1939–1943.* By MARY E. MURPHY. New York: Professional and Technical Press. 1943. xiv + 403 pp. $2.50.

This is a disappointing book. Dr. Murphy has attempted what might have been a valuable survey of the economic mechanism of the British war effort, but has evidently put it together in a violent hurry, and it has almost every conceivable defect. The arrangement of the book is disorderly to the point of chaos. Assertions are based on estimates despite the fact that actual figures appear in tables elsewhere. Variants of the same figure appear with no explanation of the differences. Innumerable individual facts are recorded three or four times—or, worse still, stated and contradicted—often in consecutive paragraphs. On a single page (267) we are told, about agricultural workers' wages: (*a*) that labourers' wages have increased by 10/- a week; (*b*) that the new minimum is 60/-; (*c*) that in December, 1941, they were double those in 1939; (*d*) that the new minimum is about 66 per cent. above pre-War; and (*e*) that they increased 14 per cent. between December, 1941, and April, 1942. Elsewhere we learn (p. 137) that they rose in amounts varying from 9/- to 17/- a week; (p. 138) that they rose in 1941 to 60/- as compared with 34/6 before the War; and (p. 139) that the increase from August, 1939, to July, 1942, was 73 per cent. The conjunction, in two separate chapters, of these eight statements, four at least of which must necessarily be wrong, is no unfair sample of Dr. Murphy's technique.

Nor are her comments much more reliable. It is sheer nonsense, for example, to say that " Not a single criticism, on the part of management or labour, has been made against the [Essential Work] Order during its operation " (p. 121) or that it is " impossible for any Briton to earn more than £3,000 a year " (p. 200). Incidentally, the inclusion of " 1943 " in the title of her book is misrepresentation; no information later than November, 1942, is given, and the bulk of the book was obviously written in the summer of 1942.

In his introduction, Dr. Finer states that Dr. Murphy's " scholarship fulfils all the requirements of so large and important a theme ". Unfortunately this is very far from the case. She has collected a lot of information, but has failed to digest it, and the result is almost useless.

J. K. HORSEFIELD.

*Food and Farming in Post-War Europe.* By P. LAMARTINE YATES and D. WARRINER. Oxford University Press. 1943. 118 pp. 3s. 6d.

" This series of books has hitherto treated of separate nations." These are the opening words of this booklet, but nothing more is said about the series or its contents except on the dust cover. Surely it would be in the interests of the publishers to give such of the readers of a book in a series like *The World To-day*, as may not know the other volumes, some information about the companion volumes otherwise than on the dust cover; most readers discard the cover either at once, or at any rate before they know if they would like to experiment further in the series.

The authors of this brief sketch of a vast subject concentrate most of their attention upon eastern Europe and rightly so, because that is where problems are most acute. They write pleasantly, keeping the human element to the fore, and do not overload their text with facts and figures; they point to the basic problems and do not let the essentials get smothered by the introduction of side issues. When it comes to the question of the policies to pursue they favour re-organised peasant farming against more drastic experiments, and they keep the psychological aspects of the situation well in mind. Thus the fact that the peasant will have had his fill of regimentation in recent years is borne in mind when treating of the measures necessary in order to improve the position of the peasant and to transform the agricultural economy of eastern and southern Europe. An enjoyable and profitable couple of hours awaits the readers of the latest addition to *The World To-day.*

A. M. CARR-SAUNDERS,

*Standards of Local Expenditure.* By J. R. and U. K. Hicks. National Institute of Economic and Social Research: Occasional Papers No. 3. Cambridge University Press. 1943. 61 pp. 4s. 6d. net.

This paper contains the first results of an enquiry into the Burden of Local Taxation undertaken by Professor and Mrs. Hicks on behalf of the National Institute of Economic and Social Research.

The problem of the Rates is by no means a new one. The real trouble is not so much the total amount of the burden, which compared, for instance, with national taxation is relatively light, but the disparity which exists between different areas. An attempt to solve this problem and at the same time to relieve the burden on the rates generally was made in the Local Government Act of 1929. That Act failed to achieve a lasting solution and while some temporary improvement followed, rate poundages had by the outbreak of war almost reached the same levels as they were before the passing of the Act. The war has temporarily attracted attention elsewhere and it has also fortuitously relieved the burden on the rates, but the problem remains and will almost certainly re-emerge after the war.

In the present paper, Mr. and Mrs. Hicks have set out to answer two questions. Firstly, why is it that the level of the rates varies so much from place to place? And secondly, why did the Local Government Act of 1929 fail to provide a remedy?

Comparative figures for rate poundages alone do not provide an adequate basis for investigation. A high poundage rate may indicate a high level of expenditure, or it may mean that the area is a poor one and has to levy a high rate to maintain even a moderate standard of expenditure. Similarly a low rate may indicate a low standard of expenditure or it may simply mean that the area is a wealthy one. Mr. and Mrs. Hicks have therefore chosen two new standards of measurement for the purposes of their investigation. The standard of expenditure they measure by reference to the amount spent per head of the population—both out of the rates and out of the block grant from the Exchequer—and wealth by reference to the yield of a penny rate. By reference to these standards, Mr. and Mrs. Hicks have classified the areas chosen into several broad groups and by analysing the expenditure of each group they have endeavoured to find the causes of high expenditure and the reasons why the high expenditure occurs where it does. While they attribute part of the responsibility to increased expenditure on education and the general health services, they conclude that the real cause lies in the burden of poor relief and they suggest as the solution the transfer of the burden to the country as a whole via the medium of a National Board of Guardians.

The paper is a valuable contribution to our knowledge and understanding of the burden of Local Taxation, and its conclusion is one which will meet with general acceptance. The nature of the remedy, however, may give rise to some disagreement especially if the proposals in the Beveridge Report—which is dealt with in a brief Epilogue—are implemented.

The investigation is confined to the County Boroughs; other boroughs and rural areas have been excluded on the ground of the difficulty in obtaining the necessary statistical material. This limitation—which is probably unavoidable—is a pity as it means that the study is a study of Urban Rating alone and the conclusions may not be equally applicable to Rural Rating. London—for some reason which has not been stated—has also been excluded.

F. A. Cockfield.

*Ceylon Currency and Banking.* By B. R. Shenoy. Longmans. 1941. 295 pp. 12s. 6d.

It is generally admitted that the British public understands distressingly little about India, despite the enormous literature (mostly available in English) dealing with various aspects of the chief present-day Indian problems. Still less is known about Ceylon, whilst in this case the literature is extremely scanty, even in the form of reports and official records. Even those who are genuinely interested in colonial policy—or in the countries of the East—are apt to ignore Ceylon, on the assumption that this island is merely a small replica of, and in some ways an appendage to, India. They fail to realise the entirely different social and historical backgrounds of the two countries, and hence the special features and problems of Ceylon. Yet Ceylon has an area of over 25,000 square miles and a population larger than that of any other part of the Empire, apart from India, Canada, the Union of South Africa, Australia and Nigeria.

It is, indeed, high time that special studies should be undertaken and interest awakened in this most attractive and, in many ways, progressive island. Mr. Shenoy's book, therefore, deserves a warm welcome. The author has made the most of the admittedly very limited material at his disposal, and has written a lucid, sensible and informative account of the development and present organisation of Ceylon's Currency and Banking systems. He explains the close relationship with and dependence upon the Indian currency system, which entailed the adoption of the Indian rupee as the standard currency up to 1939, when an Ordinance was issued which proposed the creation of a " Ceylon rupee " and the demonetisation of the Indian rupee. The implementation of this reform was postponed by the outbreak of war, and Ceylon—like most other countries—adopted exchange control. The Currency Ordinance of 1939 and the exchange control regulations are given in full in the Appendices, and Mr. Shenoy discusses at length both the provisions of the Ordinance and other possible solutions of the problem.

Mr. Shenoy also explains the (unusually simple) structure of Ceylon's banking system, emphasising the part played therein by the Imperial Bank of India, and describing the objects and establishment, in 1938, of the Bank of Ceylon. This latter is not a Central Bank, in the technical sense, but is a State-aided commercial bank "of the British type". There has been considerable discussion as to the desirability of establishing a Central Bank, and Mr. Shenoy examines the arguments advanced on either side. He concludes that in view of the strong position and on the whole satisfactory part played by the Imperial Bank of India in the financial structure of Ceylon, and the absence of a money market in the Island, there is at present no need for such a Bank.

VERA ANSTEY.

*The Conception of Surplus in Theoretical Economics.* By A. DASGUPTA. Calcutta. 1942. 214 pp. Rs. 6 or 8s.

While the renaissance in Welfare Economics, sponsored during the last few years by Professor Hicks and others, would at least call for substantial revision, Dr. Dasgupta has printed his book as it was submitted as a thesis in 1937, merely adding two appendices and "retouching" the last two chapters. One cannot deplore this too strongly, for not only does it mean that the book is not up-to-date; but also that the author has ignored the reformulation of the fundamental approach to the subject according to which the history of the Surpluses may be more significantly interpreted. Thus, Dr. Dasgupta's catholic selection of literature—ranging from the Physiocrats, Marx, A. S. Johnson to such comparatively unknown names as U. Gobbi—merely results in a potted anthology with a running commentary. It fails to trace the different trends of the Surplus idea coherently. Again, owing to the same lack of a guiding principle, the selection of the literature itself has suffered. The most serious omission, perhaps, is that of the development of the Partial Welfare analysis by the Neo-Classicals, especially by Henry Sidgwick.

The contemporary attitude[1] towards the place of Surpluses in economic theory may be summarised as follows:—

(I) Surpluses belong to the Partial Welfare Analysis, because
  (*a*) Surpluses cannot "exist" unless we can isolate the outlay and returns in a particular branch of activity from the rest of the economic system, without significant errors.
  (*b*) Surpluses are "globular" magnitudes, intractable to the General Equilibrium technique of equalising *Marginal* Social Returns.

(II) The Surplus analysis is to be adopted on pragmatic and not on logical grounds. That is to say, the Surpluses have a separate place beside the logically more stringent General Equilibrium analysis because of their immense practical usefulness in dealing with the "divergences from the Optimum" which characterise the real economic system. Owing to its lack of logical generality, the Surplus analysis, of course, has to be restricted to "glaring" cases of divergences, where the circumstances peculiar to those cases admit of a Partial analysis. But in spite of this, it still retains its usefulness within a fairly wide range.

Dr. Dasgupta, on the other hand, heroically persists throughout the greater part of his book in "interpreting" the Surpluses in a General Equilibrium framework. In this, he either verges on sophistry or leaves out the Surpluses altogether in his enthusiastic digressions on the beauties of the General Equilibrium analysis. Thus, Dr. Dasgupta adds an appendix pompously entitled "On the Ultimate Nature of Costs", extolling the "superior objectivity" of the Opportunity Cost over the Real Cost. By that addition, he somehow expects to further our understanding of the Surpluses, without leading us to the inevitable conclusion of the Opportunity Cost, *i.e.* the Knight-Davenport dictum that Surpluses do not exist. Sympathising with Professor Viner, one may be sceptical about the much vaunted "objectivity" of the Opportunity Cost approach. No doubt the Opportunity Cost implies a general comprehensive view of economic activity, while the Real Cost is inseparable from a Partial method. But, while realising the full dangers of "misplaced concreteness", it may be suggested that looking for "objectiveness" or absolute logical excellence will be but an empty academic game, if we do not first make sure what specific purposes we aim at in our theoretical constructions. It may be that the economic legislator merely wanting to know who gains or loses and how much from a tax on a given commodity need not concern himself about the ultimate nature of costs or their logically most stringent form. A crude Real-Cost Surplus approach may work, provided he make sure that the tax has no significant and widespread repercussions. Dr. Dasgupta, in "interpreting" and not demolishing the surpluses, may do better to analyse the practicable range of the Real Cost theory more elaborately instead of being carried away by the austere elegance of the Opportunity Cost.

Again, seduced by the General Equilibrium analysis, Dr. Dasgupta argues that there should be no distinction between the Producers' and Consumers' Surpluses, because, "in the ultimate analysis" the buyer is also a seller and the supply curves may be reversed into demand curves *à la* Wicksteed (p. 2). Faced with such lofty logic, one hesitates to suggest that sometimes distinctions may be drawn for the more pedestrian purpose of economic policy and that here the two sorts of surpluses have different practical significance for the following reasons:—

(1) The isolation of a supply curve of a product from the rest of the system is generally liable to greater errors than the same process done to a demand curve. Even under the favourable conditions of simple competition where complementary and competitive demands do not exist, the inter-relations on the supply side persist, presenting serious obstacles to the Partial method.

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[1] J. R. Hicks: "Rehabilitation of Consumers' Surplus," *Review of Economic Studies*, 1941.

(2) While it is plausible in some circumstances to interpret the demand curve for a commodity as the expression of ultimate consumers' satisfaction from it, the real cost involved in producing a commodity cannot be adequately expressed by the Marginal Cost Curve, if only for the chain of "an infinite variety of efforts and sacrifices", which may be traced *ad infinitum*. (*Cf.* Marshall's *Principles*, p. 852.)

(3) The assumption of constant marginal utility of money which figures so much in the Surplus analysis is again more plausible in the case of the consumer than in that of the producer, due to what Professor Hicks would call asymmetry Income Effects. Since the individual as a consumer spends only a fraction of his total income on each of the commodities he consumes, changes in his total income will not significantly affect his demand for any one of these commodities. On the other hand, the individual as a producer derives the whole of his income from one or two sources. Changes in his total income are bound to affect the sale of goods and services he offers. Thus, the marginal utility of money is not likely to be constant when we are estimating Producers' Surplus.

Ch. VII, on Marshall, is the only place in the book where Dr. Dasgupta admits the point, the neglect of which has elsewhere prevented him from coming to grips with his subject, viz., that Consumers' Surplus belongs to the Partial Analysis and that Marshall used it to point out the exceptions to the Optimum, contemplated by a General Equilibrium approach. He does not attempt to reconcile this with his earlier statement that the theory of surpluses "is essentially a corollary of the Maximum Principle" (p. 9). This chapter again has suffered, not only from the negligence of the contributions of Professor Hicks and Mr. Henderson, but also from the author's failure to appreciate Marshall's pragmatic approach. Thus Dr. Dasgupta tells us that while Dupuit appeared to seek "an objective entity", the "subjective element is primary in Marshall's representation of Consumers' Surplus; its expression in money terms is secondary" (pp. 119–120). One needs only to read the *Principles*, Ch. II, Bk. I, to realise Marshall's obsession with keeping his feet firmly on the ground and adopting the measuring rod of money as the objective basis of economic policy. In fact, Marshall even went further and depreciated the science of wants in favour of the science of activities, which to him represented the mainspring of human history. (Op. cit. at p. 90). Dr. Dasgupta should most certainly re-read his Marshall, especially the Note XIV to the Mathematical Appendix, one of the best Neo-Classical sermons explaining the pragmatic Partial method.

After this long stream of unfavourable criticism, the critic is glad to point out one interesting idea Dr. Dasgupta has traced in his Static Aspect of Distributional Surplus. By Distributional Surplus, Dr. Dasgupta means the Net Revenue which figured largely in the writings of the Physiocrats and the Ricardians. He points out that this is due to arbitrary materialistic ideas concerning the productiveness of one class of factors, the surplus being derived by subtracting the physical quantity of the "subsistence fund" from their total physical product. This point is interesting because it led the Classical Economists away from the allocated process of the price mechanism to a preoccupation with "Parsimony" which they thought would increase the volume of the employment of the Productive factors. It also inclined them to estimate the National Dividend in terms of costs. The relevance of these to modern discussions is obvious. Apart from the Bowley-Hicks arguments as to whether we should regard the prices as indicators of Marginal Utilities or Marginal Costs, the central issues of Welfare Economics at present are after all divided between those who seek prosperity by increasing the total volume of employment and those who insist that we should at least make sure whether the given volume of employment is efficiently distributed among competing uses. Dr. Dasgupta's discussions of the dynamic aspects of the Distributional Surplus, which consist merely of a summary of the profit theories of Professor Knight and fragments of the Trade Cycle theory is not helpful, if not irrelevant. One looks in vain for the mention of the difficult problems of Social Surpluses and losses from Inventions and Obsolescence.

ITLA MYINT.

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*International Agreements on Conservation of Marine Resources.* By JOZO TOMASEVICH. Food Research Institute, Stanford University, California. 1943. 297 pp. $3.00.

Dr. Tomasevich, a former economist of the National Bank of Yugoslavia, has devoted some years to a very thorough and well documented study of the efforts made, in some cases successfully, to arrive at international regulation of the resources of the sea. He has covered a wide field of interest, and his book will be of great value to fishery biologists, economists and administrators.

After a general discussion of the problems of conservation, the organisation of fisheries research, and the difficult question of territorial waters, he describes in full detail the steps taken to regulate the capture of fur seals in the North Pacific, to control the fishery for the Pacific Halibut, which is shared between the United States and Canada, and to rehabilitate the salmon fishery of the Fraser River. In dealing with these three examples of international cooperation he has performed a most useful service by weaving into a coherent story in each case the extensive but somewhat scattered literature which exists. It is not at all easy to piece together the whole history of these cases from the numerous official and other reports which have been published over a series of years, and Dr. Tomasevich's book will save subsequent enquirers a great deal of labour.

In an Appendix he refers briefly but adequately to the beginnings made in European waters towards a rational exploitation of the sea fisheries, in which the British Government has played a leading part, and has been supported by the International Council for the Exploration of the Sea. He does not, however, deal with the important discussion of the overfishing problem which took place at the Berlin meeting of the Council in May, 1939.

The Fur Seal Convention of 1911, entered into by the United States, Canada, Japan and Russia, after many years of argument and controversy, has been a very successful measure. The killing of these seals in the open waters of the North Pacific was prohibited, and power was given to the United States to regulate the killing of seals on the breeding grounds of the Pribilof Islands. The herds had been much depleted by excessive and unintelligent slaughter, but by carefully limiting the take on the breeding grounds it was steadily built up, till thirty years later it had increased tenfold. Unfortunately the treaty was formally abrogated by Japan about one year before her attack on Pearl Harbour.

Another successful experiment in conservation was initiated by the Halibut Treaty of 1930 between the United States and Canada, which empowered a joint Fisheries Commission of the two countries to limit the catch by means of a quota system and to impose other measures of protection. For years the Pacific halibut fishery had exhibited all the signs of over-exploitation, and it had been studied with the greatest thoroughness by the scientific staff of the International Commission under the distinguished leadership of Dr. W. F. Thompson. The story is typical of what happens in every unregulated fishery as it expands. The more accessible fishing grounds become depleted first, the catch per unit of effort begins to fall, and finally the total yield. This is compensated for by extending operations to more distant grounds, and by increasing the size and technical efficiency of the vessels employed, so that the total overall yield is maintained or even increased for a time. The result, in the halibut fishery, as Thompson wrote in 1936, "was a maintained total catch, hiding successive depletions of bank after bank, until the yield that came originally from an area of 500 miles was stretched over 2,000 miles of coast from Oregon to Bering Sea." Meanwhile the catch per unit of fishing power fell catastrophically in the areas first fished, and also the yield from these areas. It was clear that more weight of fish was being taken than could be replaced by the natural processes of reproduction and growth and that the stock was being forced down to ever lower levels of abundance. Unchecked increase in the intensity of fishing leads inevitably to a decrease in total catch and to increased costs of catching. There are various possible remedies, of varying effectiveness—close seasons, closed areas, size-limits, restrictions on gear, direct limitation of the amount of fishing, direct limitation of the annual catch. It was this last method which was put into force in the halibut fishery. An annual quota of catch was fixed in 1932, at such a level that the stock was given a chance to build itself up. The results were that the catch per unit of gear employed rose steadily till by 1941 it was about 80 per cent. greater than in 1930, showing that the stock of fish had greatly increased in abundance; costs of catching were reduced, and the average size of fish had increased. So far as the biological effects were concerned, the regulations were a great success, and fully came up to expectation. The quota system had some very interesting economic effects, which are fully analysed by Dr. Tomasevich. As the fish became more abundant the quota was reached more quickly and the length of the fishing season was shortened. The increased profitableness of the fishery attracted more vessels to it, and tended further to shorten the season. This concentration of landings into a shorter period had a depressing effect on prices. To counteract to some extent these somewhat adverse effects of the quota system, the industry had to exercise a measure of control, by imposing a per-man-per-trip quota on the individual vessels, and a compulsory lay-up between trips, all with the object of spreading the fishing evenly and extending the season.

Altogether, the successful American-Canadian regulation of their joint halibut fishery provides a most interesting object lesson in the merits and demerits of a quota on landings as a means of combating the evil effects of overfishing.

The overfishing problem is one which concerns our British deep-sea fisheries very closely. There is ample evidence that before the war many of our trawl fish were seriously depleted by too great an intensity of fishing, and ways and means of mitigating the evil of overfishing were being actively studied, under the auspices of the International Council for the Exploration of the Sea. The great reduction in fishing intensity brought about by the present war will inevitably result in a great increase in the stocks of trawl fish, as it did in fact in the last war. There will therefore be an excellent opportunity for action to be taken internationally to keep the stocks of fish up to a more profitable level by preventing overfishing. How this can be accomplished is of course a matter for the most careful consideration by the Governments concerned. It will not be easy, but a study of what has already been achieved in the North Pacific, as so ably set out by Dr. Tomasevich, will be distinctly helpful in deciding what measures to adopt.

E. S. Russell.

*Official Report of the Social Survey Conference, Cape Town.* By Paul Koston. Cape Town. 1942. 238 pp. 3s. 6d.

In February, 1942, a Social Survey Conference was held in Cape Town to consider the Interim Report of the Social Survey of Cape Town, conducted during the years 1936–42 by the Department of Social Science, University of Cape Town, under the direction of Professor E. Batson, who signs the Interim Report.

The most important result of this Survey has been for the first time to give quantitative expression to the very great discrepancy in standards of living between the European and the non-European sections of the population of Cape Town, each of which forms almost exactly half the city's total population of 350,000. It shows that of the Coloured (mixed) and the many fewer Native (Bantu) households, more than 50 per cent. were below a very low poverty line, while the corresponding proportion for European (white) households was under 5 per cent. More than 40 per cent. of non-European households were seriously overcrowded, as compared with about 3 per cent. of European households.

The much greater poverty of the non-European section is clearly connected with the distribution of occupations. Non-Europeans occupied only 6 per cent. of the professional, clerical and business jobs, 8 per cent. of the supervisory manual, 30 per cent. of the skilled manual, 48 per cent. of the semi-skilled manual, and 82 per cent. of the unskilled manual. They also provided 80 per cent. of the unemployed.

Finally, despite the much greater poverty of the non-Europeans, nearly twice as many non-governmental social welfare organisations were available to Europeans as to non-Europeans.

The facts provided by the Report were considerably supplemented by the papers read during the three days of the Conference. It was shown, for instance, that the immensely greater incidence of poverty among non-Europeans is due not only to their small share in the better paid types of work, but also to the fact that the gap between skilled and unskilled wages is much wider than in most other countries. As Bishop Lavis said in his paper on "The Unprotected Worker," "In America and Australia the unskilled in the building trade receive 75 per cent., in London 75 per cent., in Glasgow 74 per cent. of the skilled rates. In Cape Town they receive 26 per cent. and in other centres considerably less. In South Africa the average annual earnings of Europeans in all industries is £224 6s. od., of non-Europeans £52 1s. od. The figures in private enterprise are £287 and £52 4s. od."

The effects of this poverty were made only too clear in the papers by Professor T. S. Higgins and others on the health of the population. In 1939–40 the infant mortality rate was three times as high among non-Europeans as among Europeans, the death rate from tuberculosis six times that of Europeans, from bronchitis and pneumonia seven times, and from venereal disease and its after-effects over eight times. The total non-European death rate, including deaths from diseases, such as cancer and heart disease, which are not closely related to social conditions, was rather more than twice that of the European section.

The main direction of the Conference's discussion of the causes of and remedies for the extreme poverty of non-Europeans was indicated by Professor W. H. Hutt in his opening paper on "Industrial Legislation and Collective Bargaining", in which he contended that non-European poverty was substantially increased by the many legal, social and organisational restrictions upon the employment of non-Europeans in any except unskilled occupations. These restrictions consist, not so much of a direct legal "colour-bar", as of such measures as the fixation by trades unions or legislation of minimum wages for skilled occupations at levels which limit the demand for skilled labour to the number of European workers available; and the insistence upon qualification by apprenticeship for entry into skilled trades together with an educational qualification for admission to apprenticeship far above that which the extremely poor educational facilities open to non-Europeans enable any appreciable number of them to attain.

Professor Hutt's views received support from the majority of subsequent speakers, including Professor J. L. Gray, who, as he says in his paper on "A Labour Policy for the State", approached the subject from the point of view of "a citizen ardently wishing to bring about a socialist democracy". After salving his conscience with the *obiter dictum* that "the employer or the system of capitalist exploitation is the real villain of the piece", Professor Gray agrees that "the political influence of the white workers in maintaining the industrial colour bar and, in general, the caste system of Southern Africa, is greater than some labour apologists care to admit. . . . It is probable that for a long time to come the increase of opportunities for non-Europeans in the sphere of employment, on the basis of increasing racial equality, must procure the stabilization of skilled wage-rates, if not threaten them with a positive reduction. This is the price the white artisanocracy would pay for being taken at its word."

The conclusions of the Conference on this question are embodied in two of the most important of its nineteen resolutions. They are worth quoting in full:—

> "*Resolution* 4. That, in view of the poverty disclosed by the Survey, a Commission be appointed to investigate the practical possibility of removing restraints on equality of employment opportunities, due regard having been given to the established expectations of races, skilled workers and other economic groups".[1]
>
> "*Resolution* 12. That there be equal educational opportunities for all and compulsory education for children between the ages of seven and seventeen, due regard being paid to the adequate nourishment of children receiving such education. Furthermore, it is imperative that the Union Government make the necessary adjustments to the Financial Relations Act, permitting a far higher subsidy for the Coloured child than obtains at present."

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[1] The final clause of this resolution was carried by the Conference only after considerable discussion and on the insistence of Professor Hutt, in conformity with his view that even desirable change must not be allowed violently to disturb existing expectations.

The extent to which the Union Government takes steps to implement these resolutions may serve as evidence of the sincerity of South Africa's desire to co-operate fully in the sort of post-war world which so many are hoping for. Meanwhile, the Social Survey Conference may on the whole be congratulated on an unusually satisfactory combination of good intentions and good sense.

F. W. PAISH.

*This Age of Fable: The Political and Economic World We Live In.* By GUSTAV STOLPER. Harrap. 1943. 318 pp. 10s. 6d.

Adherents of empiricism, philosophical and otherwise, would have us believe that Man's picture of the World is an attempted generalization of his experience in it. For this to be true the average man would have to display mental alertness and intensity of intellectual effort by far surpassing anything that we observe in reality. Experience, so far from warranting the empiricist view, teaches us that most people take their world-picture ready-made from somebody else. As the number of world-pictures currently in use at each moment is limited, it follows that, for good or evil, the power wielded by the makers of such *formulæ* must be enormous. To write history in terms of them, " as seen through contemporary eyes ", as culled from newspaper front-pages, is a standing temptation to the less scholarly brand of historians. The responsible historical scholar, on the other hand, is characterized by a patient endeavour to cut through the tangle of myths and fables which the contemporary observer's need for hurried orientation has allowed to grow up, to lay bare the intricate network of human purposes and plans which forms the pattern of history, and thus to replace ideologies by ideas, to dethrone the myth-makers and give the makers of human plans, statesmen, soldiers, business leaders, their historic due.

In the present book Dr. Stolper, former editor of *Der Deutsche Volkswirt*, who has the almost unique distinction of being the only German Liberal who ever presented a liberal economic programme, makes war on the myths and myth-makers of our time, " This Age of Fable ". " The age of fable has been the age of ' debunking '. The ' debunkers ' introduced the fables. We have fallen victim to easy formulas because with the growing complexity of life we have lost faith in our ability to master it." (309)

One after another, our author takes the fables currently in fashion, economic and political, and shows them up for what they are. In this valiant struggle, hitting out impartially both Right and Left, he is brilliantly successful. The Saga of Individualism and the Collectivist Utopia, the Gold Myth and the " Production for Profit or for Use " alternative, The Inflation Complex and the tale of " Poverty amid Plenty " are all being knocked down with vigour and distinction. It is fair to add, however, that Dr. Stolper's own inclination is towards a liberal conservatism. " The radical reaches for the stars to bring the order of heaven down to our earth. The conservative is more humble. The bolder he strides, the more he strives to keep his feet on the ground, but he remembers that there is a heaven above him. The radical, forgetful of yesterday, sees only to-morrow; the conservative thinks always of yesterday when he goes about his work for to-morrow. The world picture of the radical is in black and white. The world picture of the conservative has more colours, but none of them is garish." (15)

In Part Two the contemporary fables concerning international relations are dealt with in their turn: Of Have and Have-not Nations, Of the economic causes of War, Of Britain's Degeneracy, Of British Imperialism, Of Soviet " Planning ", Of the German Miracle, Of Decadent Democracy and Efficient Dictatorship. Here the reader must bear in mind that the book was written—evidently during 1941—mainly for the enlightenment of the American public, but this in no way detracts from its value, least of all from that of the two excellent chapters on Britain and the Empire, every word of which is to-day as important a contribution to Anglo-American understanding as when it was written. The chapter on Soviet " Planning " provides plenty of food for thought for those whose enthusiasm is all too easily aroused by " facts "—preferably statistical—imperfectly understood and insufficiently analysed.

One fable is conspicuous by its absence from Dr. Stolper's enumeration: the utopia of national self-determination. One cannot but wonder why he should think that " On August 1, 1914, a world that seemed to be built for eternity went to pieces. On that day this age of fable began. We have difficulty in remembering how it was before that day, how it was to live in a world where everything was safe, certain, secure." (17). Was not this seemingly " safe, certain, secure " world of 1914 politically based on a precarious balance between two rival groups of Great Powers, and was this *de facto* order in any way compatible with the predominant ideology which was that of nationalism? Was not the Austro-Hungarian Monarchy an essential element of this order, and had not the political strength of the Monarchy for decades been sapped by the poison of nationalist agitation holding out hopes of " independent existence " to all and sundry? Here, then, is a fable which yet remains to be " debunked ", and we can but hope that one day it will be done by Dr. Stolper with the verve and poignancy of style here so brilliantly displayed.

L. M. LACHMANN.

*War Time Patterns of Saving and Spending.* By CHARLES MADGE. National Institute of Economic and Social Research: Occasional Papers No. 4. Cambridge: At the University Press. 1943. viii + 139 pp. 6s. 0d.

The National Savings Campaign has had a great deal of publicity; every Sunday evening the B.B.C. treats us to a series of figures showing the increases in the various types of saving during the preceding week ; in the press both advertising and editorial space is frequently taken up with savings news. But we know very little about the distribution of war savings by families, and in the present work Mr. Madge has tried to remedy the defect.

There are various ways in which an inquiry of this type can be undertaken. Individual savings accounts may be scrutinized and certain deductions can be drawn from them, or the interview method may be used. The first method is probably preferable and will as a general rule give more accurate results about those who save through a savings bank. It would be especially interesting if it were coupled with an investigation into the composition and income of the account-holders' families. However, it is difficult to get information about savings banks, they are bound to secrecy, their officials frequently have neither the staff nor the inclination to co-operate, and last but not least a great deal of saving is done by other methods. Mr. Madge has been able to analyze a comparatively small number of such accounts, but for the greater part of his results he has had to rely on the interview method.

In spite of the increasing popularity of this method in social investigation, there are still a good many people who regard its results, especially if unconfirmed or unconfirmable by outside evidence, with a good deal of suspicion. Those who have had even slight experience of the almost incredible stupidity with which members of the general public complete even the simplest of forms are apt to be rather sceptical when confronted with the results of fairly complicated questioning. Mere internal consistency of the results proves very little, as there may be systematic errors, although these can be reduced if experienced interviewers are used. In Mr. Madge's case, moreover, the rate of refusal in one of the surveys was fairly high, and his tests for bias in the remainder are not convincing. Although his sample agrees with the general population in respect of occupation and family composition, it remains to be demonstrated (if it can be demonstrated) that refusal to answer the questionnaire is not associated with a certain type of saving behaviour.

Having said all this, let it be admitted at once, that Mr. Madge does submit his results to various scrutinies, in order to ensure their representativeness, and it is highly probable that his results for Leeds and Glasgow are substantially correct. He concludes that there is a relatively small class of heavy savers who account for a goodly proportion of national saving. This seems highly probable, and would indicate the need for continued propaganda to stimulate those who are at present not saving enough.

Mr. Madge follows the practice of the Ministry of Labour in their enquiry into family expenditure and presents us with averages for different groups only. This is a pity, because a study of the dispersion of expenditures in different groups might have given interesting and worth-while results in return for very little additional work. In some places one can discern that Mr. Madge has been associated with Mass Observation, and it is difficult to see the reason for the inclusion of passages dealing with " The Demand for Children " and of Appendix III which contains a very vivid description of Slough and Glasgow. These would have been rather more appropriate for some of the livelier weeklies.

The author modestly describes his study as " a reconnaissance on the economic and social front ". It is hoped that the subject will be elaborated, though what seems very necessary, as a first step, is a preliminary investigation into the accuracy of the interview method in general.

E. GREBENIK.

## SHORTER NOTICES

*Economic Problems of War and its Aftermath.* Edited by CHESTER W. WRIGHT. University of Chicago Press. 1942. xi + 197 pp. $2.

This series of lectures, delivered last winter under the auspices of the Charles R. Walgreen Foundation for the Study of American Institutions, is a contribution to the education of America in the realities of war. Two of the speakers dealt with the past (the effects of war on Europe, 1540–1640, and war-planning in American history), three with the present (fiscal policy, price-control, and the suspension of democracy) and two with the future. Within the brief limits of a single lecture none of the subjects chosen could well be developed beyond a rather sketchy outline. English readers will probably find more interest in the last two—a characteristic statement of the necessity of individualism by Professor Knight, and a catholic study of the post-war prospects for democracy by Mr. M. J. Bonn—than in the preceding discussions of more specifically American interest. Professor Leland's *Next Steps in Financing the War* is, however, well garnished with useful American fiscal statistics.

*Nutrition and Size of Family.* Report on a New Housing Estate—1939. Prepared for the Birmingham Social Survey Committee by M. S. SOUTAR, E. H. WILKINS and P. SARGANT FLORENCE. London : George Allen & Unwin, Ltd. 1942. 50 pp. 1s. 6d.

This is a survey of the expenditure of 302 families living on a Birmingham housing estate on food, furniture, insurance payments, fares, etc. Expenditure is brought into relation to family needs, and there are separate analyses for families of different sizes. Whilst the results are not very different from those that previous surveys of this type have shown, they provide useful additional factual information on the topic.

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